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ART. X.—*Jeffries Wyman. Address of Professor* A. GRAY at the Memorial Meeting of the Boston Society of Natural History, Oct. 7, 1874.*

WHEN we think of the associate and friend whose death this society now deplores, and remember how modest and retiring he was, how averse to laudation and reticent of words, we feel it becoming to speak of him, now that he is gone, with much of the reserve which would be imposed upon us if he were living. Yet his own perfect truthfulness and nice sense of justice, and the benefit to be derived from the contemplation of such a character by way of example, may be our warrant for reasonable freedom in the expression of our judgments and our sentiments, taking care to avoid all exaggeration.

Appropriate and sincere eulogies and expressions of loss, both official and personal, have, however, already been pronounced or published; and among them one from the governors of that institution to which, together with our own society, most of Professor Wyman's official life and services were devoted,—which appears to me to delineate in the fewest words the truest outlines of his character. In it the President and Fellows of Harvard University "recall with affectionate respect and admiration the sagacity, patience and rectitude which characterized all his scientific work, his clearness, accuracy and conciseness as a writer and teacher, and the industry and zeal with which he labored upon the two admirable collections which remain as monuments of his rare knowledge, method and skill. They commend to the young men of the university this signal

* From the "Proceedings" of the Society.

example of a character modest, tranquil, dignified and independent, and of a life simple, contented and honored."

What more can be or need be said? It is left for me, in compliance with your invitation, Mr. President, to say something of what he was to us, and has done for us, and to put upon record, for the use of those who come after us, some account of his uneventful life, some notice, however imperfect, of his work and his writings. I could not do this without the help of friends who knew him well in early life, and of some of you who are much more conversant than I am with most of his researches. Such aid, promptly rendered, has been thankfully accepted and freely used.

Our associate's father, Dr. Rufus Wyman,—born in Woburn, graduated at Harvard College in 1799, and in the latter part of his life physician to the McLean Asylum for the Insane,—was a man of marked ability and ingenuity. Called to the charge of this earliest institution of the kind in New England at its beginning, he organized the plan of treatment and devised the excellent mechanical arrangements which have since been developed, and introduced into other establishments of the kind. His mother was Ann Morill, daughter of James Morill, a Boston merchant. This name is continued, and is familiar to us, in that of our associate's elder brother.

JEFFRIES WYMAN, the third son, derived his baptismal name from the distinguished Dr. John Jeffries, of Boston, under whom his father studied medicine. He was born on the 11th of August, 1814, at Chelmsford, a township of a few hundred inhabitants in Middlesex County, Mass., not far from the present city of Lowell. As his father took up his residence at the McLean Asylum in 1818, when Jeffries was only four years old, he received the rudiments of his education at Charlestown, in a private school; but afterward went to the Academy at Chelmsford, and, in 1826, to Phillips Exeter Academy, where, under the instruction of Dr. Abbot, he was prepared for college. He entered Harvard College in 1829, the year in which Josiah Quincy took the presidency, and was graduated in 1833, in a class of fifty-six, six of whom became professors in the university. He was not remarkable for general scholarship, but was fond of chemistry, and his preference for anatomical studies was already developed. Some of his class-mates remember the interest which was excited among them by a skeleton which he made of a mammoth bull-frog from Fresh Pond, probably one which is still preserved in his museum of comparative anatomy. His skill and taste in drawing, which he turned to such excellent account in his investigations and in the lecture room, as well as his habit of close observation of natural objects met with in his strolls, were manifested even in boyhood.

An attack of pneumonia during his Senior year in college caused much anxiety, and perhaps laid the foundation of the pulmonary affection which burdened and finally shortened his life. To recover from the effects of the attack, and to guard against its return, he made, in the winter of 1833–34, the first of those pilgrimages to the coast of the Southern States, which in later years were so often repeated. Returning with strength renewed in the course of the following spring, he began the study of medicine under Dr. John C. Dalton, who had succeeded to his father's practice at Chelmsford, but who soon removed to the adjacent and thriving town of Lowell. Here, and with his father at the McLean Asylum, and at the Medical College in Boston, he passed two years of profitable study. At the commencement of the third year he was elected house-student in the Medical Department, at the Massachusetts General Hospital,—then under the charge of Doctors James Jackson, John Ware and Walter Channing,—a responsible position, not only most advantageous for the study of disease, but well adapted to sharpen a young man's power of observation.

In 1837, after receiving the degree of Doctor of Medicine, he cast about among the larger country towns for a field in which to practice his profession. Fortunately for science, he found no opening to his mind; so he took an office in Boston, on Washington street, and accepted the honorable, but far from lucrative post of Demonstrator of Anatomy under Dr. John C. Warren, the Hersey Professor. His means were very slender, and his life abstemious to the verge of privation; for he was unwilling to burden his father, who, indeed, had done all he could in providing for the education of two sons. It may be interesting to know that, to eke out his subsistence, he became at this time a member of the Boston Fire Department, under an appointment of Samuel A. Eliot, Mayor, dated Sept. 1, 1838. He was assigned to Engine No. 18. The rule was that the first-comer to the engine-house should bear the lantern, and be absolved from other work. Wyman lived near by, and his promptitude generally saved him from all severer labor than that of enlightening his company.

The turning point in his life, i. e., an opportunity which he could seize of devoting it to science, came when Mr. John A. Lowell offered him the curatorship of the Lowell Institute, just brought into operation, and a course of lectures in it. He delivered his course of twelve lectures upon Comparative Anatomy and Physiology in the winter of 1840–41; and with the money earned by this first essay in instructing others, he went to Europe to seek further instruction for himself. He reached Paris in May, 1841, and gave his time at once to Human Anatomy at the School of Medicine, and Comparative Anatomy

and Natural History at the Garden of Plants, attending the lectures of Flourens, Majendie and Longet on Physiology, and of de Blainville, Isidore St. Hilaire, Valenciennes, Dumeril and Milne-Edwards on Zoology and Comparative Anatomy. In the summer, when the lectures were over, he made a pedestrian journey along the banks of the Loire, and another along the Rhine, returning through Belgium, and by steamer to London. There, while engaged in the study of the Hunterian collections at the Royal College of Surgeons, he received information of the alarming illness of his father; he immediately turned his face homeward, but on reaching Halifax he learned that his father was no more.

He resumed his residence in Boston, and devoted himself mainly to scientific work, under circumstances of no small discouragement. But in 1843 the means of a modest professional livelihood came to him in the offer of the chair of Anatomy and Physiology in the medical department of Hampden-Sidney College, established at Richmond, Virginia. One advantage of this position was that it did not interrupt his residence in Boston, except for the winter and spring; and during these months the milder climate of Richmond was even then desirable. He discharged the duties of the chair most acceptably for five sessions, until, in 1847, he was appointed to succeed Dr. Warren as Hersey Professor of Anatomy in Harvard College, the Parkman professorship in the Medical School in Boston being filled by the present incumbent, Dr. Holmes. Thus commenced Prof. Wyman's most useful and honorable connection as a teacher with the university, of which the President and Fellows speak in the terms I have already recited. He began his work in Holden Chapel, the upper floor being the lecture-room, the lower containing the dissecting-room and the anatomical museum of the college, with which he combined his own collections and preparations, which from that time forward increased rapidly in number and value under his industrious and skillful hands. At length Boylston Hall was built for the anatomical and the chemical departments, and the museum, lecture and working-rooms were established commodiously in their present quarters; and Prof. Wyman's department assumed the rank and the importance which it deserved. Both human and comparative anatomy were taught to special pupils, some of whom have proved themselves worthy of their honored master, while the annual courses of lectures and lessons on Anatomy, Physiology, and for a time the principles of Zoology, imparted highly valued instruction to undergraduates and others.

In the formation and perfecting of his museum—the first of the kind in the country, arranged upon a plan both physiological and morphological—no pains and labors were spared, and

long and arduous journeys and voyages were made to contribute to its riches. In the summer of 1849,—having replenished his frugal means with the proceeds of a second course of lectures before the Lowell Institute (viz., upon Comparative Physiology, a good condensed short-hand report of which was published at the time),—he accompanied Capt. Atwood of Provincetown, in a small sloop, upon a fishing voyage high up the coast of Labrador; in the winter of 1852, going to Florida for his health, he began his fruitful series of explorations and collections in that interesting district. In 1854, accompanied by his wife, he traveled extensively in Europe, and visited all the museums within his reach. In the spring of 1856, with his pupils, Green and Bancroft, as companions and assistants, he sailed to Surinam, penetrated far into the interior in canoes, made important researches upon the ground, and enriched his museum with some of its most interesting collections. These came near being too dearly bought, as he and his companions took the fever of the country, from which he suffered severely, and recovered slowly. Again, in 1858–9, accepting the thoughtful and generous invitation of Capt. J. M. Forbes, he made a voyage to the La Plata, ascended the Uraguay and the Parana in a small iron steamer which Capt. Forbes brought upon the deck of his vessel; then, with his friend George Augustus Peabody as a companion, he crossed the pampas to Mendoza, and the Cordilleras to Santiago and Valparaiso, whence he came home by way of the Peruvian coast and the Isthmus.

By such expeditions many of the choice materials of his museum and of his researches were gathered, at his own expense, to be carefully prepared and elaborated by his own unaided hands. A vast neighboring museum is a splendid example of what munificence, called forth by personal enthusiasm, may accomplish. In Dr. Wyman's we have an example of what one man may do unaided, with feeble health and feebler means, by persistent and well-directed industry, without eclât, and almost without observation. While we duly honor those who of their abundance cast their gifts into the treasury of science, let us not—now that he cannot be pained by our praise—forget to honor one who in silence and penury cast in more than they all.

Of penury in a literal sense we may not speak; for although Prof. Wyman's salary, derived from the Hersey endowment, was slender indeed, he adapted his wants to his means, foregoing neither his independence nor his scientific work; and I suppose no one ever heard him complain. In 1856 came unexpected and honorable aid from two old friends of his father who appreciated the son, and wished him to go on with his sci-

entific work without distraction. One of them, the late Dr. William J. Walker, sent him ten thousand dollars outright; the other, the late Thomas Lee, who had helped in his early education, supplemented the endowment of the Hersey professorship with an equal sum, stipulating that the income thereof should be paid to Prof. Wyman during life, whether he held the chair or not. Seldom, if ever, has a moderate sum produced a greater benefit.

Throughout the later years of Prof. Wyman's life a new museum has claimed his interest and care, and is indebted to him for much of its value and promise. In 1866, when failing strength demanded a respite from oral teaching, and required him to pass most of the season for it in a milder climate, he was named by the late George Peabody one of the seven trustees of the Museum and Professorship of American Archæology and Ethnology, which this philanthropist proceeded to found in Harvard University; and his associates called upon him to take charge of the establishment. For this he was peculiarly fitted by all his previous studies, and by his predilection for ethnological inquiries. These had already engaged his attention, and to this class of subjects he was thereafter mainly devoted,—with what sagacity, consummate skill, untiring diligence and success, his seven annual Reports—the last published just before he died,—his elaborate memoir on shell-heaps, now printing, and especially the Archæological Museum in Boylston Hall, abundantly testify. If this museum be a worthy memorial of the founder's liberality and foresight, it is no less a monument of Wyman's rare ability and devotion. Whenever the enduring building which is to receive it shall be erected, surely the name of its first curator and organizer should be inscribed, along with that of the founder, over its portal.

Of Prof. Wyman's domestic life, let it here suffice to record, that in Dec., 1850, he married Adeline Wheelwright, who died in June, 1855, leaving two daughters; that in August, 1861, he married Anna Williams Whitney, who died in February, 1864, shortly after the birth of an only and a surviving son.

Of his later days, of the slow, yet all too rapid progress of fatal pulmonary disease, it is needless to protract the story. Winter after winter, as he exchanged our bleak climate for that of Florida, we could only hope that he might return. Spring after spring he came back to us invigorated, thanks to the bland air and the open life in boat and tent, which acted like a charm; —thanks, too, to the watchful care of his attached friend, Mr. Peabody, his constant companion in Florida life. One winter was passed in Europe, partly in reference to the Archæological Museum, partly in hope of better health; but no benefit was received. The past winter in Florida produced the usual ame-

lioration, and the amount of work which Dr. Wyman undertook and accomplished last summer might have tasked a robust man. There were important accessions to the archæological collections, upon which much labor, very trying to ordinary patience, had to be expended. And in the last interview I had with him, he told me that he had gone through his own museum of comparative anatomy, which had somewhat suffered in consequence of the alterations in Boylston Hall, and had put the whole into perfect order. It was late in August when he left Cambridge for his usual visit to the White Mountain region, by which he avoided the autumnal catarrh; and there, at Bethlehem, New Hampshire, on the 4th of September, a severe hemorrhage from the lungs suddenly closed his valuable life.

Let us turn to his relations with this society. He entered it in October, 1837, just thirty-seven years ago, and shortly after he had taken his degree of Doctor in Medicine. He was Recording Secretary from 1839 to 1841; Curator of Ichthyology and Herpetology from 1841 to 1847, of Herpetology from 1847 to 1855, of Comparative Anatomy from 1855 to 1874. While in these later years his duties may have been almost nominal, it should be remembered that in the earlier days a curator not only took charge of his portion of the Museum, but in a great degree created it. Then for fourteen years, from 1856 to 1870, he was the president of this society, as assiduous in all its duties as he was wise in council; and he resigned the chair which he so long adorned and dignified only when the increasing delicacy of his health, to which night-exposure was prejudicial, made it unsafe for him any longer to undertake its duties. The record shows that he has made here one hundred and five scientific communications,* several of them very important papers, every one of some positive value; for you all know that Prof. Wyman never spoke or wrote except to a direct purpose, and because there was something which it was worth while to communicate. He bore his part also in the American Academy of Arts and Sciences, of which he was a Fellow from the year 1843, and for many years a Councillor. To it he made a good number of communications; among them one of the longest and ablest of his memoirs.

Then he was from the first a member of the Faculty of the Museum of Comparative Zoology, where his services and his advice were highly valued. He was chosen president of the American Association for the Advancement of Science for the year 1857, but did not assume the duties of the office.

Some notice—brief and cursory though it must be—of such portion of Dr. Wyman's scientific work as is recorded in his published papers, should form a part of this account of his life

* The Royal Society's Catalogue of Scientific Papers enumerates sixty-four by Prof. Wyman alone, and four in conjunction with others.

His earliest publication, so far as we know, was an article in the Boston Medical and Surgical Journal, in 1837, signed only with the initials of his name. It is upon "The indistinctness of images formed from oblique rays of light," and the cause of it. The handling of the subject is as characteristic as that of any later paper. In January, 1841, we find his first recorded communication to this society, "On the Cranium of a Seal." The first to the American Academy is the account of his dissection of the electrical organs of a new species of Torpedo, in 1843, part of a paper by his friend Dr. Storer, published in the American Journal of Science. In the course of that year, eleven communications were made to our society, besides the Annual Address, which he delivered on the 17th of May. The most important of these was the memoir, by Dr. Savage and himself, on the Black Orang or Chimpanzee of Africa, *Troglodytes niger*, published in full in the Journal of this society, the anatomical part by Professor Wyman. Two other papers of that early year, on the Anatomy of two Mollusca, *Tebennophorus Carolinensis* and *Glandina truncata*, published in the fourth volume of the society's Journal, each with a copper plate, are noteworthy, as showing that he possessed from the first that happy faculty of clear, terse, and closely relevant exposition, and that skill and neatness of illustration with his pencil, which characterize all his work, both of research and instruction.

Another paper of that year, "On the microscopic structure of the teeth of the *Lepidostei*, and their analogies with those of the *Labyrinthodonts*," read to this society in August, and published in this Journal in October, 1843, was important and timely. In it he demonstrated that the labyrinthine structure of the teeth, considered at the time to be peculiar to certain sauroid reptiles, equally belonged to the gar-fishes, and consequently that many fossil teeth which had been referred by the evidence of this character alone to a group of reptiles founded upon this peculiarity, might as well belong to ancient sauroid fishes.

Although not of any importance now to remember, I may here mention his report to this society on the *Hydrarchos Sillimani* of Koch, a factitious Saurian of huge length, successfully exhibited in New York and elsewhere under high auspices, and I think also in Germany, but which Dr. Wyman exposed at sight, showing that it was made up of an indefinite number of various cetacean vertebræ, belonging to many individuals, which (as was afterward ascertained) were collected from several localities.

But the memoir by which Professor Wyman assured his position among the higher comparative anatomists was that communicated to and published by this society in the summer

of 1847, in which the Gorilla was first named and introduced to the scientific world, and the distinctive structure and affinities of the animal so thoroughly made out from the study of the skeleton, that there was, as the great English anatomist remarked, "very little left to add, and nothing to correct." In this memoir the "Description of the habits of *Troglodytes Gorilla*" is by Dr. Thomas S. Savage, to whom, along with Dr. Wilson, "belongs the credit of the discovery;" the osteology of the same and the introductory history are by Dr. Wyman. Indeed, nearly all since made known of the Gorilla's structure, and of the affinities soundly deduced therefrom, has come from our associate's subsequent papers, founded on additional crania brought to him in 1849, by Dr. George A. Perkins of Salem; on a nearly entire male skeleton of unusual size, received in 1852, from the Rev. William Walker, and now in Wyman's museum; and on a large collection of skins and skeletons placed at his disposal in 1859 by Du Chaillu, along with a young Gorilla in spirits, which he dissected. It is in the account of this dissection that Professor Wyman brings out the curious fact that the skull of the young Gorilla and Chimpanzee bears closer resemblance to the adult than to the infantile human cranium.

In Professor Wyman's library, bound up with a quarto copy of the Memoir by Dr. Savage and himself, is a terse but complete history of this subject, in his neat and clear hand-writing, and with copies of the letters of Dr. Savage, Professor Owen, Mr. Walker, and M. du Chaillu.

In the introductory part of the Memoir, Professor Wyman states that "the specific name, *Gorilla*, has been adopted, a term used by Hanno in describing the wild men found on the coast of Africa, probably one of the species of the Orang." The name *Troglodytes Gorilla* is no doubt to be cited as of Savage and Wyman, and it was happily chosen by Professor Wyman, after consultation with his friend, the late Dr. A. A. Gould, for the reason just stated. But it is interesting to see, in the correspondence before me, how strenuously each of the joint authors deferred to the other the honor of nomenclature. Dr. Savage from first to last insists, in repeated and emphatic terms, that the scientific name shall be given by Dr. Wyman as the scientific describer, and that he could not himself honestly appropriate it. Professor Wyman, in his MSS. account, after mentioning what his portion of the Memoir was, and that "the determination of the differential characters on which the establishment of the species rests was prepared by me," briefly and characteristically adds: "In view of this last fact, Dr. Savage thought, as will be seen in letter, that the species should stand in my name; but this I declined."

This Memoir was read before this society on the 18th of August, 1847, and was published before the close of the year. But it had not, as it appears, come to Professor Owen's knowledge when the latter presented to the London Zoological Society, on the 22d of February, 1848, a memoir founded on three skulls of the same species, just received from Africa through Captain Wagstaff. When Professor Owen received the earlier Memoir, he wrote to compliment Professor Wyman upon it, substituted in a supplementary note the specific name imposed by Savage and Wyman, and reprinted in an appendix the osteological characters set forth by the latter. "It does not appear, however (adds Dr. Wyman), either in the Proceedings or the Transactions of the (Zoological) Society at what time our Memoir was published, nor that we had anticipated him in our description."

It is safe to assert that in this and the subsidiary papers of Dr. Wyman may be found the substance of all that has since been brought forward, bearing upon the osteological resemblances and differences between men and apes. After summing up the evidence, he concludes:—

"The organization of the anthropoid Quadrumana justifies the naturalist in placing them at the head of the brute creation, and placing them in a position in which they, of all the animal series, shall be nearest to man. Any anatomist, however, who will take the trouble to compare the skeletons of the Negro and Orang, cannot fail to be struck at sight with the wide gap which separates them. The difference between the cranium, the pelvis, and the conformation of the upper extremities in the Negro and Caucasian, sinks into comparative insignificance when compared with the vast difference which exists between the conformation of the same parts in the Negro and the Orang. Yet it cannot be denied, however wide the separation, that the Negro and Orang do afford the points where man and the brute, when the totality of their organization is considered, most nearly approach each other."

Selecting now for further comment only some of the more noticeable contributions to science, we should not pass by his investigations of the anatomy of the Blind Fish of the Mammoth Cave. The series began, in that prolific year, 1843, with a paper published in this Journal, and closed with an article in the same Journal in 1854. Although Dr. Tellkamph had preceded him in ascertaining the existence of rudimentary eyes and the special development of the fifth pair of nerves, yet for whole details of the subject, and the minute anatomy, we are indebted to Professor Wyman. Many of the details, however, as well as the admirable drawings illustrating them, remained unpublished until 1872, when he placed them at Mr. Putnam's

disposal, and they were brought out in his elaborate article in the "American Naturalist." Here the extraordinary development of tactile sense, taking the place of vision, and perfectly adapting the animal to its subterranean life, is completely demonstrated.

If Professor Wyman's first piece of anatomical work was the preparation of a skeleton of a bull-frog, in his undergraduate days, his most elaborate memoir is that on the anatomy of the nervous system of the same animal (*Rana pipiens*), published in the "Smithsonian Contributions," in 1852 (51 pages, royal 4to, with 2 plates).

Anything like an analysis of this capital investigation and exposition would much overpass our limits. For, although the special task he assigns to himself is the description of the nervous system of a single Batrachian, chiefly of its peripheral portion, and of the changes undergone during metamorphosis, he is led on to the consideration of several abstruse or controverted questions; such, for instance, as the attempts that have been made to homologize the nervous system of Articulates with that of Vertebrates, upon which he has some acute criticism; the theories that have been propounded respecting the functions of the cerebellum and its relation to locomotion, which he tests in a characteristic way by a direct appeal to facts; the supposition of Cuvier that the special enlargements of the spinal cord are in proportion to the force of the respective limbs supplied therefrom; which he controverts decisively by similar appeal, an extract from which I beg leave to append in a note.*

So, in describing the structure of the optic nerves in the frog, and the development of the eye and optic lobes, he proceeds to remark, that—

"The instances of *Proteus* and *Amblyopsis* naturally suggest the questions, whether one and the same part may not combine functions wholly different in different animals, and whether the

* "If by force is meant the muscular energy and development of the limbs, this statement does not appear to be sustained in the present instance, nor in many other instances brought to notice by comparative anatomy. In man the branchial enlargement is always larger than the crural, though the legs are so much more powerfully developed than the arms, and the same is true of the greater number of mammals. In frogs there is a still greater disproportion between legs and arms, yet there is not a corresponding difference in the size of the bulgings. They cannot, therefore, be said to be in proportion to the muscular force only of the limbs, but correspond far more nearly to the acuteness of the sense of touch, which in man and mammals is more delicate in the hands and arms than in the legs and feet. In bats, it is true that the muscular force of the arms is greater than that of the legs, and that the branchial far surpasses the crural enlargement; but, at the same time, the sense of touch in the membranes of the wings is exalted to a most extraordinary degree. In birds the posterior bulging is almost universally the largest, though this condition is in part dependent upon the presence of the rhomboidal sinus. In these animals, while the muscular energy of the wings is the most developed, the sensibility of the feet is the more acute."

same may not hold true with regard to the cerebral organs which is known to obtain with regard to the skeleton, the teeth, the tongue, and the nose, that identical or homologous parts in different animals may perform functions wholly distinct. If the doctrine here suggested can be admitted (and if this were the place, facts could be cited in support of it), may we not find in it an explanation of many inconsistences which now exist between the results of comparative anatomy and of physiology?"

Then, in his chapter on the philosophical anatomy of the cranial nerves and skull, after showing that there are but three pairs of cranio-spinal nerves, he takes up the controverted question as to the number of vertebræ which compose the skull, and supports the opinion that they also are only three in a characteristic manner.*

Of this whole memoir it is thought that, notwithstanding the great advance which has been made in comparative anatomy during the twenty-five years which have elapsed since it was published, its importance to the student has not at all diminished.

Next to this in extent and value may be ranked Prof. Wyman's paper on the development of the common skate of our waters (*Raia Batis*), communicated to the American Academy in 1864, and published among its Memoirs. It gives an account of the peculiar egg-case of the Selachians, and of the several stages of the development of the embryo skate, expressed in the concise and clear language—as little technical as possible—for which he was distinguished, and leading up to not a few problems in comparative anatomy, morphology, or systematic zoology,—problems which Professor Wyman never evaded when they came directly in his way, and seldom handled without making some real contribution to their elucidation. For instance, in describing the external branchial fringes of the young skate, he notes the agreement in this character with the Batrachians; and in studying the seven branchial fissures of the embryo, he is brought into contact with the view of Huxley, that the formation of the external ear is by involution of

* "The conclusions which have been drawn from the statements made above are as follows: that in frogs the *vagus* comprises the glosso-pharyngeal and accessory nerves; that the *trigeminus* comprises the facial, the abducens, and in the salamanders the patheticus and portions of the motor communis; that other evidence sustains the hypothesis, that the whole of the motor communis is a dependence of the trigeminus; if to these we add the *hypoglossus* (which in frogs is exceptionally a spinal nerve), we shall have three pairs of cranial nerves, each having all the characters of a common spinal nerve, namely, motor and sensitive roots and a ganglion; that there are no nerves to indicate a fourth vertebra, unless the special sense nerves are considered; if these are admitted as indications, then we must presuppose either two pairs of nerves to each vertebra, or the existence of six vertebræ, which is a larger number than can be accounted for on an osteological basis. The functions and mode of development of the special sense nerves we have taken as affording sufficient grounds for considering them as of a peculiar order, and not to be classed with common spinal nerves."

the integument. After confirming the contrary observations of Reichert, on the embryo pig, he concludes that "the first of the seven branchial fissures of the embryo skate is converted into the spiracle, which is the homologue of the Eustachian tube and the outer ear-canal." After a full discussion of the homology of the upper jaw in sharks and skates, under the light afforded by his investigation of the embryo skate, he suggests that the cartilage which extends from the olfactory fossæ toward the pectoral fin is the probable homologue of a maxillary bone, and that in the lobe the homologue of an intermaxillary; that if so, the skates and proteiform reptiles agree in having the nostrils open in front of the dental arch: that while in all Batrachians the nasal groove becomes closed, in the skate it remains permanently open; and finally that this view, if confirmed, "will add another feature which justifies Owen, Agassiz and others, in dissenting from Cuvier so far as to give the Selachians a place in the zoological series higher than that of the bony fishes. But at the same time, it will give corroborative proof of the correctness of Cuvier's view, that 'the rudiments of the maxillaries, and intermaxillaries, ... are evident in the skeleton.'"

[To be continued.]

ART. XI.—*On some Points in the Geology of the Blue Ridge of Virginia;* by WM. M. FONTAINE.

[Concluded from page 22.]

MY next examination was made in the vicinity of Lynchburg, sixty miles southwest of this point. There the James River, and the canal along its banks, cut the strata nearly at right angles to their strike, thus affording unsurpassed facilities for the study of the country. In order that some idea may be had of the nature of the very considerable area lying between Charlottesville and Lynchburg, I give here a section made by Prof. Rogers, across the valley, about twenty miles northeast of Lynchburg. This is the only connected examination, so far as I know, that has ever been made across this part of the valley, which includes a belt sixty miles long and twenty wide. Of course, many variations from this section are to be expected, in such an extended area. Commencing at the western base of the Blue Ridge, at "Tye River Gap," we have—I. Sandstones and slates (Silurian), with high northwest dip; width three-quarters of a mile, extending up the mountain. II. A broad range of granitic rocks; soft, and decomposing, without stratification. These continue three miles to the summit and one mile down the east side. They contain frequent beds of epi-

dotic granite, and a beautiful dark-gray syenite. The latter two are hard and permanent. In this space occur two bands of slaty rocks, one on the west side of the summit, and the other on the east side. That on the west side is one mile below the top, is 200 yards wide and contains gray and reddish slates, with associated greenstone-slate, sometimes amygdaloidal. Dip, moderate, southeast. The other band is half a mile below the summit on the east side. It presents repeated alternations of red and gray slates, white sandstones, and conglomerates, with a high dip, to the southeast, and a width of half a mile. Entire distance four miles. Down the east slope we find gray granitic rocks, with some bands of argillaceous slate, and toward the east have two or three dykes of *greenstone* (pinite porphyry ?). Extent four miles. III. From the last point, on the junction of the north and south forks of Tye River, the country up to two miles beyond Lovingston is mainly gneissic, with frequent beds of true granite near Lovingston. Dip, high, southeast. Extent fourteen miles. IV. Greenish chloritic granite; gneiss; and bluish mica slate. Dip, vertical. Interval occupied, three miles. This last is the position of Buffalo Ridge. V. Findlay's Mountain, one mile wide, composed of coarse quartzite. Dip vertical. After this the dip changes to northwest, and then to southeast, which it retains for a long distance as we pass east.

It will be noticed that in this section the quartzites composing the Ragged Mountains in the central portions of the valley are not found. In No. III the granite mentioned forms the most western ledges of the eruptive rocks to be described near Lynchburg. The chloritic granite is the representative, most probably, of the epidosites of the northeast. If so, this rock, which is *protogine*, is a metamorphic product from argillaceous slates. In the Catoctin Mountains, to the northeast of Lynchburg, various useful minerals occur; among which, copper ores, galena and magnetic iron may be mentioned. These may, however, be of Triassic age, as great masses of Triassic trap occur in them near these deposits.

Coming now to the vicinity of Lynchburg, and commencing with the examination of Buffalo Ridge, three miles southeast of the town, we find massive mica slates and schists composing the ridge and having the form of a closed anticlinal. No metamorphic core is present. Similar rocks occupy the whole distance up to the town. They are disposed in several rather open anticlinals. Near Mt. Athos, four miles down the river from the ridge, we find magnesian limestone associated with some quartzites, mica and talcose slates, containing deposits of magnetic and specular iron. Here occurs the curious rock called "catawbarite," by Lieber. This is an intimate mixture

of talcose matter with specular and magnetic iron, containing sometimes enough of the latter to be worked as an ore.

Just opposite the town of Lynchburg occurs the most eastern of the ledges of the syenitic rock, which forms an important feature of the east and central portions of the valley. Since the section given above was made by Rogers, the railroad from Washington to Lynchburg has been made. This runs for sixty miles among these ledges, and exposes by numerous cuts their nature. One of the most striking features presented is the great amount of decay exhibited. Often cuts twenty feet deep fail to give perfectly sound material. It weathers to a rusty brown mass, presenting rounded outlines. Some ledges, however, are almost perfectly fresh. The bedding, and relation to the enclosing mica slates, are well shown both here and at Lynchburg, in the natural section presented along James River.

Returning to the latter place, we find, as we proceed up stream, numerous ledges of this rock succeeding each other at short intervals, being overlaid unconformably by mica slate, which passes occasionally into mica schist. These ledges occupy a belt extending to the west some five or six miles, but diminish in frequency westward until mica slates again occupy the entire surface. These latter continue to the distance of some ten miles from the town, when the coarse granites and syenites of "Tobacco Row," and "No Business" Mountains make their appearance.

The ledges in question often present the appearance of low dykes with rounded summits, over which the slates arch unbrokenly. Then they commonly lie in heavy layers, resembling stratified beds to a certain extent. This, however, I consider to be produced by that concentric structure so often seen in cooling masses, and which, when formed on a large scale and viewed in small segments, is easily mistaken for stratification. The rock sometimes breaks through the slates, and one ledge reverses the dip developed by its neighbor, crumpling back the slates, contorting and crushing them. The width varies greatly. I saw one ledge 600 feet wide near the town, but many are no more than 100 feet wide.

The principal component is hornblende, in pretty large crystalline particles, imbedded in a rather scanty cement of fine granular, white feldspar. This is too fine to show crystalline faces, and hence the species could not be determined. Taking the features all together, it most resembles a syenite, formed by imperfect fusion through metamorphic action on some underlying rock rich in basic compounds. Its texture is rather loose, none of the components are perfectly crystallized, or fused together, and hence perhaps the ease with which it decomposes.

The following accessory minerals occur occasionally: garnet, rutile, rubellan, pyrite.

For much of the distance above Lynchburg mica slate predominates. It shows mostly a dip south-southeast, but several reversals occur, and continue for a considerable distance. The fact is to be noted, that in this region there is no constancy of dip indicating a great succession of closed folds. The mountains some ten miles above Lynchburg, containing the granites and syenites of the central parts of the valley, I had no opportunity to examine. From hand specimens obtained, the granite is an exceedingly coarse-grained, rough-looking rock, composed principally of quartz, with a smaller amount of black mica and white orthoclase. The mica has a tendency to aggregate into lumps, so as to give the rock a rather singular aspect, the white ground of the quartz and feldspar being marked with black spots, sometimes of large size. These rocks are to be regarded as forming one system with the syenites at Lynchburg, as is shown in the cuttings along the railroad, which runs through them in the valley.

My detailed examination was resumed on James River, about three miles from where it issues from the east base of the Blue Ridge. The ridge here is a composite one, being formed of several parallel chains; those on the east being composed of crystalline rocks, and those on the west of Silurian strata. The foot hills on the east often rival in height the main chain.

The first rock seen here is a mica schist, with a dip 30° northwest. This on the west is succeeded by a mass of gneiss 900 feet wide, apparently intruded among the slates. Its dip is so confused by vertical joints that I could not make it out. The composition is principally of uncrystallized feldspar, dark mica in fine lines, and quartz distributed in numberless seams of almost microscopic fineness, showing that the rock is highly altered. This is succeeded by a narrow band of mica slate, dipping southeast, toward the gneiss. The slate is succeeded by a wide belt of coarse syenitic rock, which forms some of the highest foot hills of the ridge here. This rock extends for a half mile, and is overlaid unconformably by mica slates and schists, through which it has evidently been protruded. It is a massive coarse-grained rock, composed principally of red feldspar (orthoclase), quartz, and a smaller amount of hornblende. None of the constituents are well crystallized, or distinctly segregated. The feldspar forms large particles and the hornblende shows a tendency to arrange itself in irregular patches and lines. Some greenish feldspar occurs. The junction with the slates on the west side well shows the relation of the two systems. The slates lean upon the syenite with a dip of 45° to the northwest, showing a sharply distinct line of con-

tact. This mass is plainly an outlier of a larger body of similar rock, which is to be found in the main mountain just west of it. The slates and schists continue 200 yards, when they are thrown into a gentle arch by the protrusion of a ledge of pinite porphyry, 300 feet wide. This in all respects resembles the rock at Harper's Ferry. It occupies here the same position relative to the Silurian strata, being about three miles distant from them. After an interval of 150 yards of slate, we have a second outcrop of pinite porphyry, in a ledge about eighty feet wide. This is followed by several hundred yards of mica slates, showing a moderate southeast dip. For some distance the rock is concealed, and then we have about 400 yards of argillites, the first appearance of this rock on this section. These slates do not have the heavy bedding and chloritic composition of those described farther north, but are thinly laminated and cleave well. Their western edge is thrown into a crushed anticlinal, changing from a previous dip to the southeast. This is caused by the appearance of an extensive ledge of pinite porphyry about 600 feet wide. Succeeding this we have a belt of mica slate, about half a mile wide, extending to the crystalline massive rock of the main ridge. These latter slates dip high to the southeast, being almost vertical and regulated in dip and strike by the course of the massive rock of the Blue Ridge. These dips given as southeast are more properly south-southeast and even south in some places, according to the curves of the crystalline mass which determines the position of the main chain.

A few hundred feet from the main mountain we see a ledge of highly-altered quartzite, about forty feet wide, reaching over toward the ridge. This rock is significant, for it resembles a highly indurated form of the Silurian quartzites, which are to be seen on the opposite side, arching over with a reversed dip to meet it. This ledge, however, is much thinner than the Silurian strata, and is impregnated with chlorite and epidote in places, having its texture perfectly compact. The slates between it and the syenite are much altered, being cut by joints, and having lost their slaty cleavage. They are also impregnated with siliceous and ferruginous matters, causing them to assume a red color on weathering. Their edges, viewed across the chasm cut by the stream, show this change very strikingly. They are seen to arch over the east slope of the crystalline rock in the form of a shell.

Passing through the gorge cut by the river in the most eastern of the principal elevations, we find that the river skirts its west base for a short distance. Turning here so as to front this mountain from the west, we see that its entire face, for more than 1,000 feet, is composed of a rugged, precipitous mass of

unstratified crystalline rocks, while a band of reddish rocks, eighty or one hundred feet thick, show their edges skirting the summit. This band is the west edge of the slates which were seen resting on the mountain along the east side.

The Blue Ridge in this part of the State is, as stated before, composed of several parallel ranges of nearly equal height. The one just mentioned is composed of a species of coarse syenite, while the other ranges more to the west are made up of the durable quartzites which here form the Lower Silurian strata.

The entire zone of crystalline, massive rocks here seen is about one mile wide. The most eastern portion, composing the main ridge just described, is a rock in all respects like the coarse syenite mentioned as composing the foot hills farther east. It is truly a protean rock. No two hand specimens will show exactly similar physical features, although the essential constituents are pretty constant. Red orthoclase, in coarse, badly crystallized particles, forms the chief component, but white and green feldspar of the same species also occur. Quartz is next in abundance. The hornblende, which is scanty in amount, never forms distinct crystals. Rutile not well segregated, and films of hematitic matter, are also seen. For some distance next to the slates, on the east, the rock has the fine, well-crystallized texture of a diorite. We have here without doubt an eruption of true igneous rock along the line of junction of the two systems, an occurrence not uncommon.

On the west side we see at the base of the main mass a ledge about 100 feet wide, of a rock of similar composition with the coarse syenite, but of much finer texture; it being in some parts almost compact, and impregnated in spots with chlorite and epidote, so as to give it a bright green color. This again seems to be a modification produced by its position on the border of the principal mass. West of it, the rock changes again, and for about 1,200 feet we have a variety of syenite of fine and more uniform texture, in which the minerals are more individualized, and better crystallized. This extends up to the Lower Silurian strata, which rest against it with a steep northwest dip, the lower beds being so much altered as entirely to lose their characteristic features. This portion of the syenite betrays its more durable character by presenting extensive ledges in the bed of the river, while the coarse rock to the east has been worn down almost entirely. I consider this to be the equivalent of the peculiar syenite to be described as occurring at the Peaks of Otter.

My next examination was made twenty-five miles to the southwest, along a line ten miles in length, extending westward, from Liberty in Bedford County to the Peaks of Otter.

The Catoctin Mountains, as cut by the extension of this line, do not form a continuous range. In its stead, we have isolated elevations, composed of ribs of quartzite standing nearly vertical. To the west of Liberty, for four or five miles, we have mica schists and hornblende schists, much decayed and eroded. Succeeding these come hornblende schists, presenting the same character, and all with moderate southeast dip. On reaching "Big Otter Creek," about two and a half miles from the Blue Ridge, we see a ledge of protogine, showing a width of 600 feet. This has thrown off the hornblende schist nearly vertically to the southeast. This rock, which from its position is the equivalent of the pinite porphyry, is composed of quartz, dull white feldspar, and talc, arranged with granitoid texture. The quartz often appears in shot-like lumps, and sometimes shows quite large irregular masses. It presents in this condition a peculiar sky-blue color and waxy luster. There can be no doubt, I think, that it is the same mass which appears farther north as pinite porphyry, but here like most of the rocks of this region, from more complete metamorphism, having assumed more of the character of a true igneous rock.

Next to this, on the west, we have a belt a quarter of a mile wide of heavy-bedded mica schist, which passes into a bedded syenite or hornblende rock. The exposures here are bad, and the passage from one rock to another cannot be traced, for the surface of all these rocks is much decayed and concealed by clay. Dip of both southeast. This syenite is composed of well segregated hornblende, in large crystals, combined with large crystals of a yellowish-white feldspar, too much decayed to show its true character, but apparently triclinic. This rock is succeeded by a wide belt of massive syenite, which is more igneous-looking in its durability, want of bedding, and apparent protrusion. The latter contains white orthoclase, much hornblende and a little quartz. This extensive belt reaches to within half a mile of the main mountain. It is here succeeded by a very coarse syenite, which exactly resembles that described at Balcony Falls, in the preceding section, with the exception that the red feldspar, which principally composes it, shows distinct cleavage. This extends to within a few hundred yards of the base of the mountain. The interval is occupied by a valley, in which a stream flows, whose alluvial deposits, combined with the vast quantity of transported boulders, conceal the rock. It is apparently the same with the preceding, for the principal mass of the mountain which rises from the valley is composed of the same coarse syenite, showing, however a considerable proportion of white triclinic feldspar. The topography of the locality presents some noteworthy features.

The Blue Ridge is here again a single chain, composed entirely of crystalline rock. There are no considerable hills lying to the east of the main ridge. This latter rises immediately from the valley mentioned, with a rounded, gently swelling slope, bare of surface-earth, but covered profusely with immense boulders derived from the mass composing the Peaks proper.

After attaining the summit of the slope, we find ourselves on a level with a valley lying to the west. This lies along the foot of a chain of mountains situated farther west, which rivals the Blue Ridge in height, and is composed of Silurian strata. The summit of the slope is about 1500 feet above the valley on the east.

On each side of this point rises abruptly a mountain 1600 feet above the pass. That on the right, composed of the same rock with the one on the left, is not an isolated peak, but it is the end of a great mass of mountains which extend northeast as far as the eye can reach, and even surpass in height this *peak*. The elevation on the left, which, with the one just mentioned, forms the Peaks of Otter, is an isolated eminence. It stands up abruptly from the coarse syenitic mass forming its base, like a huge chimney, showing by its bare, abrupt faces, that it is composed of different material from the softly rounded, well-worn base on which it stands. In ascending it we find the coarse syenite accompanying us to within 600 feet of the top of the crag, which forms the highest point. This would seem to indicate that in the pass at least 1000 feet of the older coarse rock has been scooped out, for the two peaks facing each other present similar features. To the southwest, the great elevation seen at the Peaks is soon lost. There the crystalline mass rapidly sinks down into several low finger-like spurs, and farther on no elevation which can be called a mountain occurs. The Virginia and Tennessee railroad passes the Blue Ridge at Buford's Gap without tunnelling, and by moderate grades. In this region, as Rogers has stated, the Silurian range to the west is called the Blue Ridge.

The following then is the structure of the mountain at the Peaks. The main mountain rises in the form of a broad, softly-rounded ridge, composed of coarse syenites, like those described at the east base, and like the rocks on James River west of Lynchburg. From this, as a base, rises abruptly on the west side near the east limit of the Silurian strata, a broad ledge of a totally different rock, which will be presently described. To this latter mass, which is about 900 feet wide, the mountains owe their additional height of 1600 feet.

Many peculiar features of erosion and the transport of matter occur, to adequately describe which too much space would

be required. These can best be explained by assuming the action of ice through this gap. In passing over the coarse decomposing syenites, which form the lower part of the mountain, we see several ledges of a much more crystalline rock penetrating them. This, which has all the characters of a true igneous substance, has made its exit principally on the west side, in the broad ledge which forms the two Peaks. It is a dark, greenish rock, very tough, and exceedingly durable. This furnishes the boulders which cover the east slopes. As the more easily decomposing, supporting framework of coarse syenite crumbled away, this more durable substance fell, and made its way down the gradual slope, perhaps aided by ice.

It is composed principally of a peculiar orthoclase of a sea-green color. This by its predominance gives a green color to the entire rock. The other constituent is a granular mixture of hornblende with a notable amount of magnetic iron. All the constituents are well crystallized, and show brilliant cleavage faces. Sometimes the feldspar occurs in quite large masses. Its crystals are always much larger than those of the hornblende and magnetic iron. Some pyrite is present. From some trials, I am inclined to think that the green color of the feldspar is due to finely disseminated hornblende. It is plainly a true igneous rock, of later date than the coarse syenites This then would be a true eruptive syenite, whose emission may have aided in producing the great disturbances seen in the Lower Silurian here and elsewhere in the State.

This belt of rocks passes out of Virginia into North Carolina, and is there continued in the broad area marked by Kerr as Upper Laurentian, in his map of that State. I will defer any remarks on the age of the different rocks here presented until after the presentation of some facts gathered from the study of the Silurian strata which join them, for these have an important bearing on this question.

In this article I have devoted most space to the geological structure of the two mountain chains and to the massive crystalline rocks, since my object in making the examination was to discover whether any igneous rocks existed in them, and, if so, to determine the part they played in their structure.

Morgantown, West Virginia, Oct. 16th.

ART. XII.—*Notice of the Chemical and Geological Essays of T. S. Hunt;** by JAMES D. DANA.

MR. HUNT has brought together, in this volume of Essays, various memoirs which have been published by him in this Journal and elsewhere. The chemical papers are important contributions to science, and show that the author was among the first to appreciate the principles which lie at the basis of what is called modern chemistry. He further applied the principles to the department of mineralogy; and in the view which he presented in 1859 with regard to the molecular relations of the feldspars, he appears to have anticipated Tschermak by ten years. The chapters on chemical geology contain much that is valuable, though not all original, on the origin of igneous and metamorphic rocks, of dolomite and gypsum, of volcanoes, of mountains, and on other topics, yet coupled with opinions, of fundamental importance, especially with reference to the making of mountains, metamorphism and the origin of some kinds of rocks, which science, we think, will never sustain. There is an important chapter on "Bitumen and Pyroschists," † pointing out the relations in chemical constitution between mineral oil and certain vegetable and animal tissues, and throwing light on the origin of the oil. The memoir on the Chemistry of Natural waters is well worthy of study. Another, on the Cambrian and Silurian in North America, is an important historical document—although failing to show that any good would come from separating the Primordial group from the Silurian and calling it Cambrian, when, so far as the fauna and stratification go, there is the closest relation between it and the overlying beds.

The reader of the volume will observe that in the Third Chapter the White Mountain series and Green Mountain series of rocks are made (as had been done by other geologists) Lower Silurian, and Upper Silurian and Devonian, in age, while in the Thirteenth Chapter (as also mentioned in the preface to Chapter III), both are pronounced pre-Silurian. In this, the older view, as I believe I have proved, is the one sustained by the facts. The new view is wholly speculative, being based on no careful stratigraphical study of the regions, but mainly upon the assumption that certain kinds of crystalline rocks are a test of geological age the world over. Since the first announcement of this doctrine by Mr. Hunt, I have spent many months in the

* Chemical and Geological Essays, By Thomas Sterry Hunt, LL.D. 490 pp. 8vo. Boston. 1875. (James R. Osgood & Co.)

† Mr. Hunt's convenient term "pyroschists," applied to shales containing carbonaceous material, is objectionable in that the rocks are shales and not schists; and on this account I have not adopted it in my Geology.

study of the Green Mountain rocks and those of some other parts of New England, in order to ascertain whether there is any virtue in the criterion; and I have found none. Mr. Hunt makes staurolite evidence of pre-Silurian age; while, as I have shown, its crystals occur in crystalline rocks of New England that are not older than Upper Silurian. Such erroneous conclusions make it apparent that in reading the work the judgment should be held in reserve until the other side is heard.

There is also another more serious reason for this reserve. For the volume contains a series of misrepresentations of the views of others wholly unnecessary to the presentation of the author's opinions, and difficult to find excuse for. These misstatements have already been the occasion of various corrections by the writer in this Journal. But, as they are now reproduced, and with additions, a few further words have become necessary.

The controversy between us, which Mr. Hunt here details so far as his side is concerned, relates chiefly to the misrepresentations of my views, as well as those of others, which are contained in his Address before the American Association in 1871, making Chapter XIII of his volume of Essays.

After many years of labor for the progress of truth, it was not a little disappointing to find myself made accountable for views I did not hold, and have them denounced as "sophistries" and "contrary jargon," even though placed under this condemnation in the good company of Gustaf Rose, Haidinger and "many others," all equally innocent of them.* Soon after receiving a copy of the Address, in the autumn of 1871, I informed Mr. Hunt of the incorrectness of his statements in long friendly letters. Obtaining no satisfaction in this, I published my review of his Address in this Journal early in 1872,† and in it denied that I entertained the views he had attributed to me, referring him to my Geology for evidence. Mr. Hunt, notwithstanding my disavowal, repeated the charges in a reply, rejecting the Geology as evidence;‡ and as a specimen of his reasoning, and his way of treating facts, I cite the following from my rejoinder:§

"Mr. Hunt's most extraordinary feat is his making out that the writer has virtually sustained the view of the *metamorphosis of granite or gneiss to limestone*, when, as I said before, it is an idea that never entered my head until the reading of Mr. Hunt's caricature of the subject. The proof which he gives is remarkable. In the first place, he says that my Mineralogy contains the fact that calcite is sometimes found pseudomorphous after quartz; and, in another place, the fact that calcite is found pseudomorphous after feldspar. Hence the conclusion, *granite or gneiss to limestone*. Q. E. D.

* Essays, p. 287. † This Journ., III, iii, 86, 1872. ‡ Ib., iv, 41, 1872. § Ib., iv, 97.

Now, if the facts respecting the pseudomorphs *were facts*, it would still require great constructive powers to make out from the statements the conclusion that I ever held to the 'metamorphosis of granite or gneiss into limestone.' But, as to the facts: (1) The Mineralogy *does not* mention any case of the pseudomorphism of calcite after quartz; and (2) the pseudomorphs of calcite after feldspar are spoken of as examples, not of an *alteration* of the feldspar, but of its removal and the substitution of calcite (4th edit., p. 249, and also 5th edit., p. 361). Now, by this substitution process, the above mentioned metamorphosis would consist (*supposing* fact No. 1 to be a fact, and that mica crystals may be similarly changed to calcite, which Mr. Hunt omits to include) in a removal of all the materials of the granite by à process of solution, and the cotemporaneous or subsequent substitution of calcite!

All will admit that the use of facts and not-facts exhibited in the above charge is most extraordinary; and can judge from it, and from other like cases stated, of Mr. Hunt's ability to appreciate, or do justice to, the views of others."

During the past year he has repeated anew his assertions, in a note before the Boston Natural History Society;* and to this I gave a brief denial in the last volume of this Journal on pages 221, 222.

Among the various other persons persistently misrepresented by him, no one has been more grossly so than the late Dr. Naumann. Mr. Hunt claimed in his Address that Naumann agreed with him in his doctrine that pseudomorphism in the case of certain silicates was simply "envelopment." I showed, by a direct citation of facts from Naumann's work on Mineralogy, that he held, on the contrary, the views of the men Mr. Hunt had denounced, and I proved that the letter from Naumann to Delesse, which Mr. Hunt cited to sustain his opinion, did not refer at all to the point in question. (See page 91 of vol. iii, 1872, of this Journal.) Naumann sustained my statement in an indignant note to the *Jahrbuch für Mineralogie*, of which the following is a translation:

Dresden, November 17, 1872.

Professor Sterry Hunt, in his Address before the American Association of the 16th of August, 1871, has quoted extracts of a letter of mine to Delesse, which were printed in the *Bulletin de la Société Géologique*, vol. xviii, p. 678 of the Second Series, and which seem to show that my views agree with his theory of the formation of serpentine and the allied rocks.

It is certainly true that in that letter I have expressed myself satisfied with this: that my friend Delesse, in his Treatise (*Recherches sur les Pseudomorphoses, Ann. des Mines*, V, xvi, 317, et seq.), has separated many examples of what are really cases merely of

* Proceedings for 1874, p. 334.

regular envelopment, (*enveloppemens avec orientation*) from the *pseudomorphs* with which, although having nothing in common with them, they had been associated. In connection, I took occasion to remark that it seemed to me an *analogous* error, when *all* gneisses, amphibolites, and so on, are regarded as metamorphic and not originally-formed rocks; and that the confounding of the two ideas of pseudomorphism and metamorphism has had many unfortunate results; and further that I recognize as pseudomorphs only those cases in which the form of the altered crystal is still retained. The mixture or mutual envelopment of two minerals is generally due indeed to simultaneous crystallization; and yet there are cases of envelopment which may be regarded as pseudomorphs, provided the form of the enveloped crystal is still recognizable.

Although I have several times opposed the exaggerated use of the doctrine of metamorphism, yet it is still incomprehensible to me how Professor Sterry Hunt can consider the portions of my letter to Delesse, quoted by him, as proofs that I regard those cases of pseudomorphism, upon which the theory of metamorphism is based, as for the most part only examples of association and envelopment as well as results of a simultaneous and original crystallization, or that this view of mine is identical with that proposed by himself in the year 1853.*

Nothing but an incomprehensible misunderstanding can explain such an opinion, which, moreover, has been already sufficiently disposed of by Dana in the American Journal of Science for February and August of 1872. CARL NAUMANN.

In Mr. Hunt's new book that letter to Delesse is again appealed to, to show that Naumann holds what he rejects, and what every one who is acquainted with his Mineralogy knows that he has never held; for the work does not contain a word on "envelopment" in the chapter on Pseudomorphism, and both in that chapter and elsewhere he presents the ordinarily accepted view. Naumann's note to the *Jahrbuch* was not called out by anything I had written him; I never addressed a letter on the subject to anyone in Europe.

Mr. Hunt endeavors to make out that Naumann knew the contents of his Address only through my "misleading criticisms," and that therefore his letter is not to be taken as meaning what it says. But Naumann, after giving his view of his own letter, is equally precise in his statement of Mr. Hunt's doctrine. Moreover, Naumann shows that he *did* have Mr. Hunt's writings before him; for he refers to Mr. Hunt's views in "1853," as quoted

*The sentence of Mr. Hunt's, published in 1853, which is here referred to, is as follows: "The generally admitted notions of pseudomorphism seem to have originated in a too exclusive plutonism, and require such varied hypotheses to explain the different cases that we are led to seek for some more simple explanation, and to find it, in many instances, in the association and crystallizing together of homologous and isomorphous species."—*Am. J. Sci.*, II, xvi, 218.—EDS.

in his Address, (see foot-note to the preceding page,) to which date or citation I have at no time until now made any allusion; and the fact that Mr. Hunt, as he admits, omitted part of the letter, is sufficient to explain Naumann's reference to *extracts*, if it be not also true that only *extracts* were published in the *Annales des Mines*. Naumann was too thorough, accurate, and conscientious a man in all his scientific work to have written that criticism of Mr. Hunt without having read carefully Mr. Hunt's writings on the subject. "There is a confusion, not to say contradiction, in these expressed views of the venerable teacher not easy to explain,"* if we take Mr. Hunt as our expositor. But all is clear enough in the words and the works of Naumann.

Mr. Hunt, in his new volume, instead of offering an apology for his misrepresentations, gives the following excuse for his course:

"Nothing has been further from my intention than to misrepresent the views either of Naumann or of Dana; and my error, if I have fallen into one, arises from the difficulty of knowing their real opinions upon the matters in discussion. Let Professor Dana define, as clearly as I have done, his present views as to the origin of magnesian rocks, both those made up of chrysolite and pyroxenic minerals, and those composed of serpentine, steatite and chlorite, which he has supposed to come from an epigenesis of the former; let him tell us whether he holds the doctrine of pseudomorphic metamorphism which he taught in 1845, 1854 and 1858, and which, as I have shown, was held by Delesse as late as 1857, or that doctrine so long maintained by me, which the latter adopted in 1861. Such a definition would be eminently satisfactory to those who look to him as a teacher in science, and would prevent any further misconception or unintentional misrepresentation of his views."

Such questions, after all that I have written on the subjects mentioned, are strangely out of place, and wrong in their implications; for I have expressly stated my present relation to the views I held in 1845, 1854 and 1858, *in my Notices of his Address;* and the difficulty complained of in the opening part of the paragraph is, as shown beyond, wholly a subjective one.

When I referred Mr. Hunt for my present opinions to my Manual of Geology (published in 1862), where they are drawn out at length, and denied that I held, or the book contained, the doctrine that "metamorphism was pseudomorphism on a broad scale," and did the same again in my rejoinder, I supposed that I had been as explicit as possible; and yet I am asked to make another assertion on the subject. In both of my notices of Mr. Hunt's Address (see the citations beyond,) I have stated

* Hunt's Essays, p. 323.

that I had regarded serpentine and some other hydrous magnesian rocks as examples of pseudomorphism on a broad scale, but I have not since 1858 made the principle a general one, or applied it to any other rocks. That statement occurs only in a book-notice in 1858.*

With regard to magnesian rocks, I have stated my views in my first notice of his Address. Again, in my rejoinder to his reply, in August, 1872 (this Journ., III, iv, 103) I observe as follows, after a mention of various facts:

"In view of such facts, the writer still holds, as in 1845, that—

* * * "The same causes that have originated the steatitic scapolites, occasionally picked out of the rocks, have given magnesia to whole rock-formations, and altered throughout their physical and chemical characters. If it be true that the crystals of serpentine are pseudomorphous crystals, altered from chrysolite, it is also true, as Breithaupt has suggested, that the beds of serpentine containing them are likewise altered; though often covering square leagues in extent, and common in most primary formations. The beds of steatite, the still more extensive talcose formations, contain everywhere evidence of the same agents."—*This Journ.*, xlviii, 92, 1845.

"Besides this paragraph, expressive of my views, Mr. Hunt cites also another of the same purport from my Mineralogy of 1854, and in this, also, I see little to modify. It is as follows: that—

"The various examples of pseudomorphism should be understood as cases not simply of alteration of crystals, but in many instances of changes in beds of rock. [Delesse admits this; see p. 99.] Thus all *serpentine*, whether in mountain-masses, or the simple crystal, has been formed through a process of pseudomorphism, or in more general language, of metamorphism; the same is true of *other magnesian* rocks, as steatitic, talcose or chloritic slates.† Thus the subject of metamorphism, as it bears on all crystalline rocks, and of pseudomorphism, are but branches of one system of phenomena."—*Min.*, *4th edit.*, i, 226, 1854.

These statements respecting hydrous magnesian rocks ought to be considered clear and satisfactory; for besides declaring my present relation to the views I had expressed in 1845, 1854 and 1858, I stated, with regard to the origin of these rocks, all that I thought to be warranted in the existing state of the science. I have nowhere attempted an explanation of the precise chemical processes in the production of magnesian rocks, while Mr. Hunt has; but I fully believe that his very clear expositions of the subject, which he presents to me as a model, will prove to be almost worthless when the "real" facts are better understood. Believing much in facts, I have been studying the subject the past year; and the observations which I have made at Brewster, New York, and which are published in the last volume of this

* Amer. Journ. Sci., II, xxv, 445. That the expression was a hasty one on my part is evident from the entire rejection of the opinion from my Geology, which was written in 1859 and 1862, and the additional fact that when I read it in Mr. Hunt's Address I could not at first believe that I was its author, and again and again hunted for it before I found it.

† I would now remove from this list talcose slate, as it has been shown to be mostly hydromica slate, and also chloritic slate, which is an ordinary metamorphic rock.

Journal (see pages 371 and 477) enable us to move a step forward, and, at the same time, leave little room for doubt with regard to the small value of Mr. Hunt's speculations.*

I had no occasion to speak of the origin of chrysolite rocks, nor do I now see that it was necessary. Such rocks are known to be very common among the oldest terranes of the globe, so that this material for making serpentine was then abundantly supplied. Chrysolite rocks and chrysolite altered to serpentine, as Genth has shown, occur in North Carolina "on a broad scale." Hornblende and pyroxene rocks are ordinary metamorphic rocks. A very pure hornblende rock of great thickness occurs in the Helderberg formation of the Connecticut Valley, in Bernardston, Massachusetts, and Vernon, Vermont.

Mr. Hunt keeps up his misrepresentations even when he is saying that "nothing is further from my intention than to misrepresent the views either of Naumann or of Dana." His plea (in the very next sentence to this one about his "intention,") as to the "difficulty of knowing their real opinions upon the matters in discussion" is a subterfuge that can do him no service. Mr. Hunt's persistent neglect to look into the works of Naumann for his opinion on the point in dispute, or to refer to them in any way, is a natural source for the difficulty he complains of. His singular refusal to take my Manual of Geology as evidence with regard to my views since 1862 is also a positive source of difficulty. But difficulty from such sources is no excuse. It may be said that the words "difficulty of knowing their real opinions upon the matters in discussion," referred especially to *one* question—the chemical process in the formation of magnesian rocks. But, as regards Naumann, this question has not been in discussion; and, besides, the expression admits of no such restriction.

In order that the absurdity of the claim of "difficulty" may be still better appreciated, and to prevent, if possible, its repetition, I here state that—

I have never held, and my writings no where sustain, the following opinions which Mr. Hunt has attributed to me and others:—

1. The "possibility of converting almost any silicate into any other."
2. The possibility of converting granite into limestone.
3. The possibility of converting gneiss into limestone.
4. The possibility of converting diorite into limestone.

* Mr. Hunt, in his hypothesis, attributes the origin of beds of serpentine and steatite to the alteration of chemically-deposited beds of different hydrous magnesian silicates related to meerschaum. Serpentine occurs of various ages, and is even found in Cretaceous rocks in California, as shown by Whitney. Certainly if serpentine had been made from the alteration of beds of hydrous magnesian silicates, such beds should now exist somewhere in the *unaltered* strata between the Archæan and Cretaceous; and yet they do not.

5. The possibility of converting granite into serpentine.
6. The possibility of converting granulite into serpentine.
7. The possibility of converting gneiss into serpentine.
8. The possibility of converting diorite into serpentine.
9. The possibility of converting limestone into granite.
10. The possibility of converting limestone into gneiss.

Again, with the exception of the year 1858, I have never held nor taught that metamorphism is pseudomorphism on a broad scale. My Geology gives a very different definition of metamorphism.

I regret that I have been compelled to return to this unpleasant subject. Charged with holding views which I did not entertain, it became a duty to the cause of scientific truth to put in a disclaimer. And now that Mr. Hunt's misstatements have been given new currency by a republication of them, with additions, in his volume of Essays, and since his repeated assertions have led to my being quoted for the views attributed to me, a new explanation and denial seemed to be demanded.

The case is a strange one in the annals of science. Four years have not sufficed to secure a recognition of the facts. Any other person, with hardly an exception, if he had had my denial, and had been referred to publications of mine that gave my views and fully sustained the denial, would have accepted the statement, and made a public correction. The misrepresentations are a blot on the volume of Essays, and one which might have easily been avoided.

ART. XIII.—*Do Varieties wear out, or tend to wear out?* By Professor ASA GRAY.

The following article was published in the N. Y. Tribune, (the Semi-weekly, of Dec. 8,) in response to some questions referred to the author. Although cast in a popular form, for general readers, we deem it well worthy of reproduction in this Journal.—EDS.

THIS question has been argued from time to time for more than half a century, and is far from being settled yet. Indeed, it is not to be settled either way so easily as is sometimes thought. The result of a prolonged and rather lively discussion of the topic about forty years ago in England, in which Lindley bore a leading part on the negative side, was, if we rightly remember, that the nays had the best of the argument. The deniers could fairly well explain away the facts adduced by the other side, and evade the force of the reasons then assigned

to prove that varieties were bound to die out in the course of time. But if the case were fully re-argued now, it is by no means certain that the nays would win it. The most they could expect would be the Scotch verdict, "not proven." And this not because much, if any, additional evidence of the actual wearing out of any variety has turned up since, but because a presumption has been raised under which the evidence would take a bias the other way. There is now in the minds of scientific men some reason to expect that certain varieties would die out in the long run, and this might have an important influence upon the interpretation of the facts that would be brought forward. Curiously enough, however, the recent discussions to which our attention has been called seem, on both sides, to have overlooked this matter.

But, first of all, the question needs to be more specifically stated if any good is to come from a discussion of it. There are varieties and varieties. They may, some of them, disappear or deteriorate, but yet not wear out— not come to an end from any inherent cause. One might even say, the younger they are the less the chance of survival unless well cared for. They may be smothered out by the adverse force of superior numbers; they are even more likely to be bred out of existence by unprevented cross-fertilization, or to disappear from mere change of fashion. The question, however, is not so much about reversion to an ancestral state, or the falling off of a high-bred stock into an inferior condition. Of such cases it is enough to say that, when a variety or strain, of animal or vegetable, is led up to unusual fecundity or of size or product of any organ, for our good, and not for the good of the plant or animal itself, it can be kept so only by high feeding and exceptional care; and that with high feeding and artificial appliances come vastly increased liability to disease, which may practically annihilate the race. But then the race, like the bursted boiler, could not be said to wear out, while if left to ordinary conditions, and allowed to degenerate back into a more natural, if less useful state, its hold on life would evidently be increased rather than diminished.

As to natural varieties or races under normal conditions, sexually propagated, it could readily be shown that they are neither more nor less likely to disappear from any inherent cause than the species from which they originated. Whether species wear out, i. e., have their rise, culmination, and decline from any inherent cause, is wholly a geological and very speculative problem, upon which, indeed, only vague conjectures can be offered. The matter actually under discussion concerns cultivated domesticated varieties only, and, as to plants, is covered by two questions.

First, *will races propagated by seed*, being so fixed that they come true to seed, and purely bred, (not crossed with any other sort,) continue so indefinitely, or *will they run out in time*—not die out, perhaps, but lose their distinguishing characters? Upon this, all we are able to say is that we know no reason why they should wear out or deteriorate from any inherent cause. The transient existence or the deterioration and disappearance of many such races are sufficiently accounted for otherwise; as in the case of extraordinarily exuberant varieties, such as mammoth fruits or roots, by increased liability to disease, already adverted to, or by the failure of the high feeding they demand. A common cause, in ordinary cases, is cross-breeding, through the agency of wind or insects, which is difficult to guard against. Or they go out of fashion and are superseded by others thought to be better, and so the old ones disappear.

Or, finally, they may revert to an ancestral form. As offspring tend to resemble grand-parents almost as much as parents, and as a line of close-bred ancestry is generally prepotent, so newly originated varieties have always a tendency to reversion. This is pretty sure to show itself in some of the progeny of the earlier generations, and the breeder has to guard against it by rigid selection. But the older the variety is—that is, the longer the series of generations in which it has come true from seed—the less the chance of reversion: for now, to be like the immediate parents, is also to be like a long line of ancestry; and so all the influences concerned—that is, both parental and ancestral heritability—act in one and the same direction. So, since the older a race is the more reason it has to continue true, the presumption of the unlimited permanence of old races is very strong.

Of course the race itself may give off new varieties: but that is no interference with the vitality of the original stock. If some of the new varieties supplant the old, that will not be because the unvaried stock is worn out or decrepit with age, but because in wild nature the newer forms are better adapted to the surroundings, or, under man's care, better adapted to his wants or fancies.

The second question, and one upon which the discussion about the wearing out of varieties generally turns, is, *Will varieties propagated from buds, i. e., by division, grafts, bulbs, tubers and the like, necessarily deteriorate and die out?* First, Do they die out as a matter of fact? Upon this, the testimony has all along been conflicting. Andrew Knight was sure that they do, and there could hardly be a more trustworthy witness.

"The fact," he says, fifty years ago, "that certain varieties of some species of fruit which have been long cultivated cannot now be made to grow in the same soils and under the same mode of

management, which was a century ago so perfectly successful, is placed beyond the reach of controversy. Every experiment which seemed to afford the slightest prospect of success was tried by myself and others to propagate the old varieties of the apple and pear which formerly constituted the orchards of Herefordshire, without a single healthy or efficient tree having been obtained; and I believe all attempts to propagate these varieties have, during some years, wholly ceased to be made."

To this it was replied, in that and the next generation, that cultivated vines have been transmitted by perpetual division from the time of the Romans, and that several of the sorts, still prized and prolific, are well identified, among them the ancient Græcula, considered to be the modern Corinth or Currant grape, which has immemorially been seedless; that the old Nonpariel apple was known in the time of Queen Elizabeth; that the White Beurré pears of France have been propagated from the earliest times; and that Golden Pippins, St. Michael pears, and others said to have run out, were still to be had in good condition.

Coming down to the present year, a glance through the proceedings of pomological societies, and the debates of farmers' clubs, brings out the same difference of opinion. The testimony is nearly equally divided. Perhaps the larger number speak of the deterioration and failure of particular old sorts; but when the question turns on "wearing out," the positive evidence of vigorous trees and sound fruits is most telling. A little positive testimony outweighs a good deal of negative. This cannot readily be explained away, while the failures may be, by exhaustion of soil, incoming of disease, or alteration of climate or circumstances. On the other hand, it may be urged that, if a variety of this sort is fated to become decrepit and die out, it is not bound to die out all at once, and every where at the same time. It would be expected first to give way wherever it is weakest, from whatever cause. This consideration has an important bearing upon the final question, are old varieties of this kind on the way to die out on account of their age or any inherent limit of vitality?

Here, again, Mr. Knight took an extreme view. In his essay in the Philosophical Transactions, published in the year 1810, he propounded the theory, not merely of a natural limit to varieties from grafts and cuttings, but even that they would not survive the natural term of the life of the seedling trees from which they were originally taken. Whatever may have been his view of the natural term of the life of a tree, and of a cutting being merely a part of the individual that produced it, there is no doubt that he laid himself open to the effective replies which were made from all sides at the time, and have lost none of

their force since. Weeping willows, bread-fruits, bananas, sugar-cane, tiger-lilies, Jerusalem artichokes, and the like, have been propagated for a long while in this way, without evident decadence.

Moreover, the analogy upon which his hypothesis is founded will not hold. Whether or not one adopts the present writer's conception, that individuality is not actually reached or maintained in the vegetable world, it is clear enough that a common plant or tree is not an individual in the sense that a horse or man, or any one of the higher animals, is—that it is an individual only in the sense that a branching zoophyte or mass of coral is. *Solvitur crescendo:* the tree and the branch equally demonstrate that they are not individuals, by being divided with impunity and advantage, with no loss of life, but much increase. It looks odd enough to see a writer like Mr. Sisley reproducing the old hypothesis in so bare a form as this: "I am prepared to maintain that varieties are individuals, and that as they are born they must die, like other individuals." "We know that oaks, Sequoias and other trees live several centuries, but how many we do not exactly know. But that they must die, no one in his senses will dispute." Now what people in their senses do dispute is, not that the tree will die, but that other trees, established from cuttings of it, will die with it.

But does it follow from this that non-sexually propagated varieties are endowed with the same power of unlimited duration that are possessed by varieties and species propagated sexually—i. e., by seed? Those who think so jump too soon at their conclusion. For, as to the facts, it is not enough to point out the diseases or the trouble in the soil or the atmosphere to which certain old fruits are succumbing, nor to prove that a parasitic fungus (*Peronospora infestans*) is what is the matter with potatoes. For how else would constitutional debility, if such there be, more naturally manifest itself than in such increased liability or diminished resistance to such attacks? And if you say that, anyhow, such varieties do not die of old age—meaning that each individual attacked does not die of old age, but of manifest disease—it may be asked in return, what individual man ever dies of old age in any other sense than of a similar inability to resist invasions which in earlier years would have produced no noticeable effect? Aged people die of a slight cold or a slight accident, but the inevitable weakness that attends old age is what makes these slight attacks fatal.

Finally, there is a philosophical argument which tells strongly for some limitations of the duration of non-sexually-propagated forms, one that probably Knight never thought of, but which we should not have expected recent writers to overlook. When Mr. Darwin announced the principle that cross-fertilization be-

tween the individuals of a species is the plan of nature, and is practically so universal that it fairly sustains his inference, that no hermaphrodite species continually self-fertilized would continue to exist, he made it clear to all who apprehend and receive the principle, that a series of plants propagated by buds only must have weaker hold of life than a series reproduced by seed. For the former is the closest possible kind of close breeding. Upon this ground such varieties may be expected ultimately to die out; but "the mills of the gods grind so exceeding slow" that we cannot say that any particular grist has been actually ground out under human observation.

If it be asked how the asserted principle is proved or made probable, we can here merely say that the proof is wholly inferential. But the inference is drawn from such a vast array of facts that it is well nigh irresistible. It is the legitimate explanation of those arrangements in nature to secure cross-fertilization in the species, either constantly or occasionally, which are so general, so varied and diverse, and we may add so exquisite and wonderful, that, once propounded, we see that it must be true. What else, indeed, is the meaning and use of sexual reproduction? Not simply increase in numbers; for that is otherwise effectually provided for by budding propagation in plants and many of the lower animals. There are plants, indeed, of the lower sort, in which the whole multiplication takes place in this way, and with great rapidity. These also have sexual reproduction; but in it two old individuals are always destroyed to make a single new one! Here propagation diminishes the number of individuals 50 per cent. Who can suppose that such a costly process as this, and that all the exquisite arrangements for cross-fertilization in hermaphrodite plants, do not subserve some most important purpose? How and why the union of two organisms, or generally of two very minute portions of them, should re-enforce vitality, we do not know and can hardly conjecture. But this must be the meaning of sexual reproduction.

The conclusion of the matter from the scientific point of view is, that sexually propagated varieties, or races, although liable to disappear through change, need not be expected to wear out, and there is no proof that they do; but, that non-sexually propagated varieties, though not liable to change, may theoretically be expected to wear out, but to be a very long time about it.

ART. XIV.—*Communications from the Laboratory of Williams College;* by IRA REMSEN.

I. *Formation of Paratoluic Acid from Parasulphotoluenic Acid.*

IN an article published a short time ago in this Journal * I stated that when potassic parasulphobenzoate is fused with sodic formate, according to the method of V. Meyer for the synthesis of aromatic acids, terephtalic acid is formed. The reaction was a very clean one, not a trace of the isomeric phtalic acids being produced. I also called attention to the fact that a certain degree of importance was connected with this result, in as much as it showed, more conclusively than had been shown up to that time, that the reaction of Meyer takes place in a normal manner, and is hence adapted to the determination of the constitution of aromatic bodies, notwithstanding the doubts that had been expressed by chemists as to its value for this purpose.

Since the publication of the above article, V. v. Richter † has attempted to prove that Meyer's reaction does not take place in a normal manner, by showing that, when benzoic acid alone is fused with sodic formate, both isophtalic and terephtalic acids are produced. He had evidently not read my article, as it appeared in this Journal, but only a brief preliminary notice which I had previously published ‡ in Germany, in which, however, I had distinctly stated that pure terephtalic acid is formed from parasulphobenzoic acid. As this notice was a *brief* one, v. Richter takes the liberty of doubting the assertion clearly made in it. After remarking that the direct replacement of the brom- and sulpho-groups in benzoic acid, by means of the questionable reaction, can only then be looked upon as proved, after it has been shown that the isomeric substituted benzoic acids may be converted into the corresponding dicarbonic acids by the same reaction, he says: "Dieses ist aber trotz verschiedener Versuche nicht gelungen; es existirt nur eine kurze Anzeige von I. Remsen dass er aus der Parasulfobenzoesäure reine Terephtalsäure erhalten;" and he then proceeds to reason exactly as if this brief notice did not exist. Subsequently, V. Meyer § answered Richter in an entirely satisfactory manner, upholding the correctness of my assertion, and employing it as an argument in favor of his views. I have hence not felt called upon to say a word in my own defense, and I now only take this opportunity to remark that *a priori* I attach as much weight to

* Vol. v, 179, 274, 354.
† Berliner Berichte, vi Jahrgang, 876, 879.
‡ Ibid, v Jahrgang, 379.
§ Berliner Berichte, vi Jahrgang, 1146.

an assertion made in a brief notice as to one made in a long article, and I shall continue to do so hereafter, whatever deviation from this principle the custom of a few chemists may appear to sanction.

In view of the importance of the question under discussion, I have endeavored to furnish further proofs of the value of Meyer's reaction, by employing it for the purpose of effecting new conversions of known compounds into known compounds the constitutions of which are established. Of one of these conversions, viz., that of parasulphotoluenic acid into paratoluic acid, I wish to speak here.

Pure potassic parasulphotoluenate was mixed with an equal quantity of sodic formate, and the mixture then gradually heated to fusion, the mass being constantly stirred. All the phenomena mentioned by Meyer, and afterward noticed by myself, were repeated. Vapors of an exceedingly disagreeable odor were given off, and the mass turned darker and darker until it became almost black. Toward the end of the operation, inflammable gases escaped and took fire at the top of the crucible. The whole was now allowed to cool and then treated with water. With the exception of a small black residue, the mixture dissolved entirely, forming a clear solution. This was treated with sulphuric acid until it showed an acid reaction: and the solution was then agitated with ether until the latter no longer extracted any solid substance. On now evaporating the ether, an almost colorless solid residue remained which exhibited the properties of an acid. It was comparatively easily soluble in boiling water, and separated from this solution, on cooling, in the form of microscopic needles. These were found to sublime with ease, and were thus obtained in the form of beautiful, flat needles. The pure substance fused instantly at 176°. It was converted into the barium salt, and from this reprecipitated. Its fusing point was again found to be 176°.

These properties suffice to characterize the substance as paratoluic acid; but another and more positive proof of its nature was given. The acid was treated with the ordinary oxidizing mixture of potassic dichromate, sulphuric acid and water, and, after boiling for a short time, an insoluble powder was thrown down. This was filtered off, washed and dried, and was then found to have all the properties of terephtalic acid. It was insoluble in water, hot as well as cold, and did not fuse, but sublimed in a capillary tube before an applied flame.

Now as parasulphotoluenic acid is known to yield a cresole, which in turn yields paraoxybenzoic acid, and as paraoxybenzoic and paratoluic acids are conceded to belong to the same series, it follows that, in the instance here considered, the sulpho-group is replaced directly by carboxyl. But, further, para-

toluic acid was the only product of the reaction. I was unable to discover a trace of any other acid, although special precautions were taken to prevent errors of observation in this respect. The quantity of the product, too, was sufficient to warrant the conclusion that it was not formed by any secondary reaction.

The results of this experiment then serve to strengthen the conclusion already drawn with reference to Meyer's reaction, and to weaken the arguments of v. Richter. I shall next attempt to convert orthosulphotoluenic acid into orthotoluic acid by the same method, though success is hardly to be anticipated, as Meyer himself failed to convert chlorsalylic acid into the corresponding orthocarbonic acid, the substituting group being replaced by hydrogen instead of by carboxyl.

Another experiment with which I have been occupied, closely allied to that described, has not yet led to decisive results. I stated, in the article already referred to, that having failed to obtain orthosulphobenzoic acid from orthosulphotoluenic acid by oxidation with potassic dichromate and sulphuric acid, I intended to use potassic permanganate in alkaline solution for the purpose of effecting this oxidation. My main object in preparing orthosulphobenzoic acid was, through it, to reach phtalic acid by means of Meyer's reaction. It appears up to the present that orthosulphotoluenic acid is destroyed entirely by potassic permanganate in alkaline solution, though I do not feel prepared to assert this positively. W. Weith* has lately shown that orthotoluic acid may be readily converted into phtalic acid by potassic permanganate in alkaline solution. Though this interesting discovery renders the preparation of phtalic acid, by the method proposed by me, unnecessary from one stand-point, I shall, nevertheless, endeavor to accomplish this.

II. *On Nitroparasulphobenzoic Acid.*

In continuing my investigations on parasulphaminbenzoic acid, I have reached some new results which are herewith communicated.

I first attempted to introduce the group SO^2OH into parasulphaminbenzoic acid in order thus to obtain a sulpho acid which, were it susceptible to ordinary reagents, might lead to definite conclusions concerning the dioxybenzoic acids and the tricarbonic acids of benzine. Common concentrated sulphuric acid dissolves parasulphaminbenzoic acid when heated with it, but does not change it, for, on diluting with water the solution thus obtained, the original acid is precipitated in its characteristic acicular crystals. The same is true in regard to the

* Berliner Berichte, vii Jahrgang, 1057.

action of sulphuric anhydride; solution is effected, but from this solution parasulphaminbenzoic acid is precipitated unchanged on the addition of water. On boiling the acid with Nordhausen sulphuric acid, however, a change is accomplished, though not that which was expected and desired. The change consists simply in a regeneration of parasulphobenzoic acid from the amine-acid. In other words, the group NH^2 is converted into OH. The parasulphobenzoic acid was recognized by diluting with water, neutralizing with baric carbonate, evaporating, adding chlorhydric acid and allowing to cool. Thus were deposited the characteristic flat needles of the acid barium salt. This possessed all the properties of the salt, and its identity was further proved by the following analysis:

0·2475 grams salt lost 0·022 grams on being heated to 200°; and gave 0·0963 grams $BaSO^4$ = 0·05662 grams Ba.

	Calculated.		Found.
$2(C^7H^5SO^5)$	402	67·79	——
Ba	137	23·10	22·88
$3H^2O$	54	9·11	8·88
	593	100·00	

I next endeavored to introduce the group NO^2 into parasulphaminbenzoic acid. Fuming nitric acid was first employed for this purpose. When this is boiled with the amine-acid, solution takes place, and, on diluting this solution with water, nothing is precipitated. The change effected is, however, the same as that effected by Nordhausen sulphuric acid, viz: the NH^2 group is simply converted into OH, and parasulphobenzoic acid thus regenerated. The latter acid was in this instance also readily recognized. The solution was evaporated to dryness on the water bath and all nitric acid thus removed. On now dissolving in water, neutralizing, as above, with baric carbonate, adding chlorhydric acid and allowing to cool, the acid barium salt was deposited. It was identified beyond a doubt by the aid of the following analysis:

0·5325 grams salt lost 0·0483 grams on being heated to 200°; and gave 0·2095 grams, $BaSO^4$ = 0·1232 grams Ba.

	Calculated.		Found.
$2(C^7H^5SO^5)$	402	67·79	——
Ba	137	23·10	23·11
$3H^2O$	54	9·11	9·07
	593	100·00	

These experiments then only show that parasulphobenzoic acid is an exceedingly stable compound, as it resists the action

of Nordhausen sulphuric acid and fuming nitric acid at the boiling temperatures, no matter how long the boiling may be continued. It may, further, be looked upon as an interesting result that the Nordhausen acid should cause a conversion of the NH^2 group into OH; a fact which at least indicates a remarkable predisposition on the part of the amine-acid to conversion into the corresponding sulpho-acid.

Finally, by employing a mixture of the two agents already employed, I was enabled to obtain a substitution-product of parasulphobenzoic acid. The boiling was continued until red fumes ceased to be given off, and the solution became perfectly clear. On now diluting with water, neutralizing with baric carbonate, etc., a new salt was obtained. This was comparatively easily soluble in hot water, though less so in cold water. It crystallized in long, lustrous needles, arranged concentrically in groups which might be termed fan-shaped. They were colored yellow, and repeated recrystallizations failed to remove this color. The analyses gave the following results, showing the salt to be the neutral barium salt of nitroparasulphobenzoic acid:

I. 0·2345 grams salt lost 0·0147 grams at 200°; and then gave 0·1320 grams $BaSO^4$ = 0·077616 grams Ba.

II. 0·2335 grams salt lost 0·0155 grams at 200°; and then gave 0·1325 grams $BaSO^4$ = 0 0779 grams Ba.

	Calculated.		Found.	
$C^7H^3SNO^7$	245	59·90	——	——
Ba	137	33·50	33·10	33·37
$1\frac{1}{2}H^2O$	27	6·60	6·27	6·64
	409	100·00		

The formula of this salt is thus seen to be

$$C^6H^3(NO^2)\begin{cases}SO^2 . O\\CO . O\end{cases}>Ba + 1\tfrac{1}{2}H^2O.$$

A nitrosulphobenzoic acid has already been prepared by Limpricht and Uslar.* But, as this was obtained from metasulphobenzoic acid, it is not at all probable that the two are identical. Nothing is known concerning the relative position of the nitro-group in either of these acids, nor have we as yet any data that will enable us to draw conclusions of value with reference to this point. In this connection, the following method of investigation suggests itself. Three isomeric nitrobenzoic acids are known, and the series to which each belongs is also known. Now, if it be possible to introduce into these nitro-acids the sulpho-group, compounds would be obtained

* Annalen der Chemie u. Pharmacie, cvi, 27.

which would probably be identical with the two known forms of nitrosulphobenzoic acid. According to the conditions then, we would have data which would enable us to determine the constitution of these latter acids; for we should thus fix the position, first, of the sulpho-group with reference to the carboxyl, and second, that of the nitro-group in one and the same compound.

I shall endeavor to prove the feasibility of this method of investigation, as soon as possible. It is well to remember, however, that Mulder* has already tried to obtain a nitrosulphobenzoic acid by the action of sulphuric acid on ordinary (meta) nitrobenzoic acid, and that no such product resulted. Possibly the isomeric nitrobenzoic acids may yield more satisfactory results.

III. *On the Action of Potassium on Ethyl succinate.*

Several years ago Fehling† described a peculiar compound which he had obtained by the action of potassium on ethyl succinate. This compound, according to the discoverer, crystallizes in yellowish laminæ which are insoluble in water, difficultly soluble in cold alcohol, easily in hot alcohol. The formula deduced from the analyses was $C^6H^8O^3$, or ethyl succinate minus the constituents of one molecule of alcohol. The exact nature of this body was not determined, nor was much more than its existence and composition proved. Its peculiar nature led me to desire a more thorough knowledge of its relations to other known compounds, and I have hence undertaken its study in the hope of being able to discover its constitution by the aid of appropriate experiments. It appears possible that it may be the representative of a new class of bodies of an interesting nature, which may be prepared under similar circumstances from the various ethers of bibasic fatty acids.

Its preparation is comparatively simple, and the yield is satisfactory. I have prepared a considerable quantity, and have found the statements of Fehling concerning it essentially correct. In a pure condition, it forms long, flat, beautiful needles of a slightly yellowish color, instead of yellow laminæ. It may be boiled with alcohol or water without undergoing change. When sodium-amalgam is added to the alcoholic solution, a very rapid change takes place. A bright red precipitate is formed, which is insoluble in alcohol but easily soluble in water. From the aqueous solution of this new substance chlorhydric acid throws down a white precipitate, which is insoluble in water and difficultly soluble in alcohol. Dilute caustic potassa produces a similar change. The substance dissolves in this very

* Annalen der Chemie u. Pharmacie, xxxiv, 297.
† Ibid, xlix, 192.

easily, and the solution conducts itself in the same manner as that above mentioned. The action of the amalgam is then due alone to the formation of caustic soda, and not to the action of nascent hydrogen.

In order to obtain the red substance above referred to, the original body was dissolved in warm alcohol, and to this solution an alcoholic solution of caustic soda was added. In this manner a bright red voluminous precipitate was obtained, which consisted of very fine lustrous needles. This was filtered off, well washed out with alcohol, pressed with bibulous paper and allowed to dry. In drying, the red color changes to yellow, but the needles remain otherwise unchanged. An addition of alcohol to the yellow substance restores the red color, so that there seems to be no chemical change connected with that of color. Further, this yellow body after being dried may be preserved in the air. Analyses gave as a result 12·37 per cent sodium, but I shall not attempt to deduce a formula until this result has been confirmed by other means.

The precipitate that is produced by chlorhydric acid in the aqueous solution of the red substance proved to be the body $C^6H^8O^3$, so that it would seem that this body plays the part of an acid. Alkaline carbonates, however, do not act in the same manner as the hydroxides. Without the aid of heat, indeed, they do not act at all. When the body $C^6H^8O^3$ is boiled with sodic carbonate, succinic acid is regenerated and is precipitated on the addition of chlorhydric acid to the cooled solution. The same is true when in the case of the hydroxides the solutions are boiled. Very concentrated aqueous ammonia does not act upon the substance $C^6H^8O^3$, even when the two are boiled together.

I intend to study the action of phosphoric chloride and acetyl chloride, and, further, of nascent hydrogen in acid solution, produced by tin and alcoholic chlorhydric acid, upon the body under investigation, and expect in this manner to reach definite results which will lead to a knowledge of its constitution.

October, 1874.

Art. XV.—*On the Detection of Hydrocyanic Acid;* by M. Carey Lea, Philadelphia.

1. *New Test.*

If a little pure protosalt of iron (I have used ferrous ammonia sulphate) be dissolved together with a little uranic nitrate, we have a solution which, with a soluble cyanide, gives a purple precipitate, or in very dilute solutions of the cyanide, a greyish purple.

This test is very delicate. Used in the manner to be presently described, a solution of potassic cyanide, containing only $\frac{1}{3\,6}\frac{1}{6\,6}$ of a grain of anhydrous hydrocyanic acid, gives a perfectly distinct reaction. It is therefore not exceeded in delicacy by any known test for that substance.

The solution of iron and uranium must not be acidulated, but on the contrary should be quite neutral, and so dilute as to be nearly colorless. A grain or two of each salt may be dissolved in a half ounce of water. Two or three drops of the mixed solution are to be placed in a clean white porcelain capsule, and a drop or two of the liquid to be tested should be made to slip slowly down, that the reaction at the point of contact may be carefully noted.

Cobaltous nitrate may be substituted for the uranium salt, and gives an almost equally delicate reaction, but the color of the cobalt salt is an objection.

2. *Prussian Blue Test.*

The delicacy of the Prussian blue reaction for the detection of hydrocyanic acid has been much understated. Thus Taylor (quoted in Watts' Dict., ii, 219) affirms that whilst the sulphocyanide reaction is capable of distinctly indicating the presence of $\frac{1}{3\,6}\frac{1}{3\,6}$ of a grain of anhydrous prussic acid, the Prussian blue test will not detect less than $\frac{1}{7\,8}\frac{1}{6}$ of a grain. A much greater delicacy than this can be obtained by using appropriate precautions.

There must not be too much iron salt present, and the solution of iron salt must not be too strong: these two points are essential. For ferric salt, the ammonia citrate is to be preferred to ammonia ferric alum or ferric chloride. The best mode of proceeding is as follows:

A weak solution of iron is to be made, containing a ferrous salt, to which a little ferric ammonia citrate is to be added. Of this solution, acidified with hydrochloric acid, two or three drops only are to be placed in a white porcelain capsule. A drop of the liquid to be tested is then allowed to slip down the side of the capsule, and this meeting the iron solution will give rise to the production of a blue cloudiness.

If a grain of potassic cyanide be dissolved in four ounces of pure water, rendered slightly alkaline with caustic alkali, and a single drop of the solution be allowed to flow down the capsule in the manner just described, a distinct blue coloration will result. This drop will have contained about $\frac{1}{2\,6}\frac{1}{6\,6}$ of a grain of cyanide, or about $\frac{1}{3\,6}\frac{1}{6\,6}$ of a grain of anhydrous prussic acid, a degree of delicacy very far surpassing that found by Mr. Taylor; surpassing, indeed, that claimed by him for the sulphocyanhydric test.

The advantage of using ferric ammonia citrate over ferric ammonia alum may be curiously shown as follows:

Place in a small white porcelain basin about an ounce of a solution containing a few grains each of ferrous ammonia sulphate and ferric ammonia sulphate acidulated with hydrochloric acid. Take about a milligramme of cyanide of potassium, or about as much dust of that substance as can be distinctly perceived and shake it into the basin. Each infinitesimal particle will produce a blue coloration as it touches the liquid, but on agitating, the blue (if the quantity of cyanide has been small enough) will disappear. If now the same experiment be repeated with a solution of ferrous ammonia sulphate and ferric ammonia citrate, also acidulated, the blue color produced does not disappear on shaking, but presently settles or becomes more conspicuous.

For the same reason, ferric ammonia citrate will detect infinitesimal traces of potassic ferrocyanide in the ferridcyanide when ferric chloride and ferric ammonia alum will not, and therefore it should have the preference for testing the purity of potassic ferridcyanide.

ART. XVI.—*Brief Contributions to Zoology from the Museum of Yale College.* No. XXX.—*The Gigantic Cephalopods of the North Atlantic;* by A. E. VERRILL.

THE existence of several distinct species of gigantic ten-armed cephalopods, belonging to more than one genus, has been well established by the researches of Steenstrup, Harting and others.

More recently attention has been repeatedly called to the frequent occurrence of these remarkable animals in the waters of Newfoundland. In an article on this subject, in this Journal, vol. vii, p. 158, Feb., 1874, I was able to enumerate five specimens from American waters, concerning which we had some reliable information.* Since that time much more material has been accumulated, and I am now able to cite twelve American examples. I have also had opportunities to study portions of five of these specimens. These evidently represent two distinct species, both of which belong to the genus *Architeuthis* of Steenstrup (or *Megaloteuthis* of Kent). The largest of these is represented only by the jaws of two specimens, one of which (No. 1 in my former article) was found floating at the Banks of Newfoundland, and the other (which

* See articles on this subject by the writer, in the American Naturalist, vol. ix, Jan., 1875; and vol. viii, p. 167; and letters from Mr. Alexander Murray in the Naturalist, vol. viii, p. 120, Feb., 1874.

we will designate as No. 10) was taken from the stomach of a sperm whale. The upper jaw of the latter was imperfectly figured by Dr. Packard in his article on this subject.* It is the largest jaw yet known. These belong to an apparently undescribed species, which I propose to name *Architeuthis princeps*,† and shall describe more fully farther on.

The second species, which I consider identical with the *Architeuthis monachus* of Steenstrup, is represented by parts of three individuals, and seems to be the species most commonly met with on the coasts of Newfoundland and Labrador.

The most complete specimen that has ever come under scientific observation was captured in November, 1873, at Logie Bay, near St. John's, Newfoundland. It became entangled in herring-nets and was secured by the fishermen with some difficulty, and only after quite a struggle, during which its head was badly mutilated and severed from the body, and the eyes, most of the siphon-tube, and the front edge of the mantle were destroyed. Fortunately this specimen was secured by the Rev. M. Harvey of St. John's. After it had been photographed and measured, he attempted to preserve it entire in brine, but this was found to be ineffectual, and after decomposition had begun to destroy some of the most perishable parts, he took it from the brine and, dividing it into several portions, preserved such parts as were still undecomposed in strong alcohol. These various portions are now in my possession, and with the photographs have enabled me to present a restoration, believed to be quite accurate, of the entire creature (plate II, fig. 1). In this figure the eyes, ears, siphon-tube, and front edge of the mantle have been restored from a small squid (*Loligo pallida*), to which this gigantic species seems to be nearly allied in many respects. The other parts have been drawn directly from the photographs and specimens.‡

Mr. Harvey has published popular accounts of this specimen and the previously captured arm of a still larger one, in the Maritime Monthly Magazine of St. John, N. B., for March, 1874, and in several newspapers.§ To him we are, therefore,

* American Naturalist, vol. vii, p. 91.

† This species was named and characterized in a communication made to the Connecticut Academy of Sciences, Nov. 18, 1874, and will be described in greater detail in its Transactions. See also American Naturalist, Feb., 1875.

‡ The figure was originally made, from the photographs only, by Mr. P. Rœtter, of the Museum of Comparative Zoology, but after the arrival of the specimens it had to be altered in many parts. These necessary changes were made by the writer, after a careful study of the parts preserved, in comparison with the photographs and original measurements.

§ Acknowledgments are also due to Mr. Alexander Murray, Provincial Geologist, who coöperated with Mr. Harvey in the examination and preservation of these specimens, and who has also written some of the accounts of them that have been published. See also the American Naturalist, vol. viii, p. 122, February, 1874; American Journal of Science, vol. vii, p. 160; Nature, vol. ix, p. 322, February 26, 1874; and Appleton's Journal, Jan. 31, 1874.

mainly indebted for the latest and most important additions to our knowledge of these remarkable animals. The preserved parts of this specimen (No. 5) which I have been able to examine are as follows: the anterior part of the head, with the bases of the arms, the beak, lingual ribbon, etc.; the eight shorter arms, but without the suckers, which dropped off in the brine, and are now represented only by the strong marginal rings; the two long tentacular arms, which are well preserved, with all the suckers in place; the tail; portions of the "pen" or internal shell; the ink-bag; and pieces of the body.

The general appearance and form of this species * are well shown by plate II. From the great size of the large suckers on the long arms, I judge it to be a male. The body was relatively stout, and according to the statement of Mr. Harvey, it was, when fresh, about seven feet long and five and one-half feet in circumference. The "tail" or caudal fin (plate IV, fig. 9) is said by Mr. Harvey to have been 22 inches across, but the preserved specimen is considerably smaller, owing, undoubtedly, to shrinkage in the brine and alcohol. It is remarkable for its broad sagitate form. The posterior termination is unusually acute and the lateral lobes extend forward considerably beyond their insertion. In the preserved specimen the total length, from the anterior end of the lateral lobes to the tip of the tail, is 23 inches; from the lateral insertions to the tip 19 inches; from the dorsal insertion 13·5 inches; total

* Mr. W. Saville Kent, from the popular descriptions of this species, has seen fit to give it new generic and specific names, viz: *Megaloteuthis Harveyi*, in a communication made to the Zoological Society of London, March 3, 1874 (Proceedings Zool. Soc., p. 178; see also Nature, vol. ix, p. 375, March 12, and p. 403, March 19). My identification is based on a comparison of the jaws with the jaws of *A. monachus*, well figured and described by Steenstrup in proof-sheets of a paper which is perhaps still unpublished, though printed several years ago, and referred to even by Harting. Their agreement is very close in nearly all respects, but the beak of the lower jaw is a little more divergent in Steenstrup's figure. His specimen was a little larger than the one here described and was taken from a specimen cast ashore in 1853. Mr. Kent was probably unaware of that specimen when he said (Nature, ix, p. 403) that *A. monachus* "was instituted for the reception of two gigantic Cephalopods, cast on the shores of Jutland in the years 1639 and 1790, and of which popular record alone remains."

His statement that *Architeuthis dux* Steenstrup is known from the beak alone appears to be erroneous, for Steenstrup, Harting, and Dr. Packard, in their articles on this subject, all state that the suckers, parts of the arms, and the internal shell or pen were preserved, and they have been figured by Prof. Steenstrup; Harting has also given a figure of the lower jaw, copied from a figure by Steenstrup. Steenstrup also mentions having the arm-hooks (Tandvæbningen), which would indicate a genus distinct from our species; in the proof-sheets which I have seen, this specimen is referred to as "*A. Titan*," but Harting cites it as *A. dux* Steenstrup. Possibly two distinct species are confounded under this name.

Should the *Architeuthis dux* prove to belong to a genus distinct from this, it might perhaps be taken as the type of *Architeuthis*, and in that case the generic name given by Kent could be retained, and the two species here described would then be called *Megaloteuthis monachus* and *M. princeps*, if my identification of the former species be correct.

breadth about 15 inches; width of lateral lobes 6 inches. The eight shorter arms, when fresh, were, according to Mr. Harvey's measurements, six feet long and all of equal length, but those of the different pairs were respectively ten, nine, eight and seven inches in circumference. They are three-cornered or triquetral in form and taper very gradually to slender acute tips. Their inner faces are occupied by two alternating rows of large obliquely campanulate suckers, with contracted apertures surrounded by broad, oblique, marginal rings, armed with strong, acute teeth around their entire circumference, but largest and most oblique on the outside (plate III, fig. 10). These suckers gradually diminish in size to the tips of the arms, where they become very small, but are all similar in form and structure. The largest of these suckers are said by Mr. Harvey to have been about an inch in diameter, when fresh. The largest of their marginal rings in my possession are ·65 of an inch in diameter at the serrated edge, and ·75 beneath. The rings of the smaller suckers are more oblique and more contracted at the aperture, with the teeth more inclined inward, those on the outside margin being largest. The two long tentacular arms are remarkable for their slenderness and great length when compared with the length of the body. Mr. Harvey states that they were each 24 feet long and 2·75 inches in circumference when fresh. In the brine and alcohol they have shrunk greatly, and now measure only 13·5 feet in length, while the circumference of the slender portion varies from 2·25 to 3 25 inches. These arms were evidently highly contractile, like those of many small species, and consequently the length and diameter would vary greatly according to the state of contraction or relaxation. The length given (24 feet) probably represents the extreme length in an extended or flaccid condition, such as usually occurs in these animals soon after death. The slender portion is three-cornered or triquetral in form, with the outer angle round, the sides slightly concave, the lateral angles prominent, and the inner face a little convex and generally smooth.

The terminal portion, bearing the suckers, is 30 inches in length and expands gradually to the middle, where it is 4·5 to 5 inches in circumference (6 inches when fresh), and 1·5 to 1·6 across the inner face. The sucker-bearing portion may be divided into three parts. The first region occupies about seven inches, in which the arm is triquetral, with margined lateral angles, and gradually increases up to the maximum size, the inner face being convex and bearing about forty irregularly scattered, small, flattened, saucer-shaped suckers, attached by very short pedicels, and so placed in depressions as to rise but little above the general surface. They have narrow marginal

rings, with the thin edges nearly smooth, or minutely denticulate, and ·10 to ·12 of an inch in diameter, surrounded by a thick and prominent marginal membrane. These suckers are at first distantly scattered, but become more crowded, finally covering the whole width of the inner face, which becomes here 1·6 inches broad. Scattered among the suckers are about as many low, broad, conical, smooth, callous verrucæ, or wart-like prominences, rising above the general surface, their central elevation corresponding in form and size to the apertures of the adjacent suckers. These, without doubt, are intended to furnish secure points of adhesion for the corresponding suckers of the opposite arm, so that, as in some other genera, these two arms can be fastened together at this wrist-like portion, and thus they can be used unitedly. By this means they must become far more efficient organs for capturing their prey than if used separately. Between these smooth suckers and the rows of large ones there is a cluster of about a dozen small suckers, with serrate margins, mostly less than a quarter of an inch in diameter, attached by slender pedicels.

The second division, 14 inches in length, succeeds the small suckers. Here the arm is well rounded on the back and flattened on the face, where it bears two alternating rows of very large serrate suckers, and an outer row of small ones on each side, alternating with the large ones. The inner edge is bordered by a rather broad, regularly scalloped, marginal membrane, the scallops corresponding to the large suckers. On the other edge there is a narrower and thinner membrane, which runs all the way to the tip of the arm. In one of the rows of large suckers there are eleven, and in the other ten, above half an inch in diameter, but each row has at either end one or two smaller ones, from half an inch to a quarter of an inch in diameter. The largest suckers (plate IV, fig. 11, *a*) are from 1 to 1·15 inches in diameter at the margin. These are attached by strong, though slender, pedicels, so that their margins are elevated about an inch above the surface of the arm. Each one is situated in the center of a pentagonal depressed area, about an inch across, bounded by ridges, which alternate regularly, and interlock on the two sides, so as to form a zigzag line along the middle of the arm. These large suckers are obliquely campanulate; the marginal ring is strong, and sharply serrate all around. The small marginal suckers (fig. 11, *b*) are similar in structure, but more oblique, and mostly only ·3 to ·4 of an inch in diameter; they are attached by much longer and more slender pedicels, and their marginal teeth are relatively larger and more incurved, especially on the outer margin. The terminal division of the arm is 9 inches long. It gradually becomes much compressed laterally, and tapers regularly

to the tip, which is flat, blunt, and slightly incurved. Just beyond the large suckers, where this region begins, the circumference is 3·5 inches. The face is narrow and bears a large number of small serrate and pedicellate suckers, arranged in four regular alternating rows, and gradually diminishing in size to the tip of the arm, where the rows expand into a small cluster. These suckers are much like the marginal ones of the previous division, and at first are about ·25 of an inch in diameter, but decrease to about ·10 of an inch near the tip of the arm. The color, where preserved, is pale reddish, with thickly scattered small spots of brownish red.

The form of the jaws of this specimen is well shown by plate III, figs. 3 and 4. When in place, these jaws constitute a powerful beak, looking something like that of a parrot or hawk, except that the upper jaw shuts into the lower, instead of the reverse, as in birds. The color is dark brown, becoming almost black toward the tip, where its substance is thicker and firmer, and smoothly polished externally. The upper jaw (plate III, fig. 3) measures 3·85 inches in total length; 1 inch in greatest breadth; and 2·50 from front to back. The lower jaw (fig. 4) is 3 inches long, 2·75 broad, and 2·65 from front to back.

The small squids of our coast have a very similar pair of jaws. Those of *Loligo pallida* (plate IV, figs. 5, 5a) are here figured twice the natural size, for comparison and to explain the terms used in describing the large jaws.

The most remarkable anatomical character observed in this specimen is found in the form and arrangement of the teeth on the "lingual ribbon," or *odontophore*, for in this respect it differs widely from all other known Cephalopods.

The ordinary squids and cuttle-fishes all have these teeth arranged in seven regular longitudinal rows; those of the three middle rows being generally two or three-pronged, as in *Loligo pallida* (plate IV, fig. 7), while the lateral rows have long, simple, fang-like teeth. But in this species (fig. 6) the teeth are very irregularly scattered over the surface of the broad thin membrane, and it is difficult to trace the rows, if such they can be called, for the arrangement seems to be somewhat in irregular quincunx. The number of rows, however, cannot be less than twenty. These teeth are all simple, but vary considerably in size and form. They are all attached by a more or less rounded, flattened base, and all are considerably curved; some are broad and tapering; others are slender and acute; but the different forms and sizes are irregularly intermingled across the whole breadth of the membrane. Irregular granules of silica are also scattered in great numbers over the membrane among the teeth, and similar grains are embedded in the membrane lining the mouth. This peculiar type of dentition must be regarded as an extremely generalized one.

The portions of the pen in my possession belong mostly to the two ends, with fragments from the middle region, so that although neither the actual length nor the greatest breadth can be given, we can yet judge very well what its general form and character must have been. It was a broad and thin structure, of a yellowish brown color, and translucent. Its anterior portion (plate III, fig. 2) resembles that of *Loligo*, but its posterior termination is entirely different, for instead of having a regular lanceolate form, tapering to a point at the posterior end, as in *Loligo*, it expands and thins out toward the posterior end, which is very broadly rounded or irregularly truncate, fading out insensibly, both at the edges and end, into soft membrane. The anterior end, for about an inch and a half, was rapidly narrowed to a pen-like point, as in *Loligo;* from this portion backward the width gradually increases from 1·2 inches to 5 inches, at a point .5 inches from the end, where our specimen is broken off; at this place the marginal strips are wanting, but the width is 5 inches between the lateral midribs (*d*, *d''*), which were, perhaps, half an inch from the margin. Along the center of the shell, there is a strong, raised, rounded midrib, which fades out a short distance from the posterior end, but is very conspicuous in the middle and anterior sections. On each side of the midrib is a lateral rib of smaller size. These at first diverge rapidly from the central one, and then run along nearly parallel with the outer margin and about ·4 of an inch from it, but beyond 11 inches from the point the margins are torn off. Like the midrib the lateral ribs gradually fade out before reaching the posterior end; near the place where they finally disappear, they are about six inches apart.

The pen of our *Architeuthis* seems to resemble that of the ancient genus *Teudopsis*, found fossil in the Jurassic formations.

From the above description it will be seen that the most important and most characteristic features of this species, or rather of the *genus* to which it belongs, are to be found in the *lingual dentition*, in the *internal shell*, in the *form of the caudal-fins*, and in the cluster of small suckers and tubercles on the long arms. As already stated, the first three of these peculiarities indicate a low or generalized structure, and therefore a low rank in our system of classification, unless it should be found to have some other characters not yet known and of greater importance, which might outweigh those here given. It will appear, therefore, that this genus of huge squids should be classed below *Loligo*, which, in its turn, would go below *Ommastrephes*, to which genus the common small squids of our northern coasts belong, for the latter genus has distinct eyelids, which are not found in *Loligo*, and the internal shell is also more specialized.

I have received through Professor Baird, of the Smithsonian Institution, a pair of jaws and two large suckers of the long arms, which were taken from a specimen (No. 4), cast ashore in Bonavista Bay, Newfoundland. These jaws agree precisely in form and size with those described above, so that the size of these two individuals must have been about the same. The suckers (plate IV, figs. 12, 13) had been dried, and have lost their true form, but the marginal rings are perfect, and only ·92 of an inch in diameter, and though somewhat smaller than in the specimen just described, they have the same kind of denticulation around the margin. Their smaller size may indicate that the specimen was a female, but they may not have been the largest of those on the arm.

EXPLANATION OF PLATES.

Plate II.—Figure 1. *Architeuthis monachus* Steenstrup (No. 5); $\frac{1}{12}$ natural size.

Plate III.—Figure 2. Anterior part of the "pen" of the same; $\frac{1}{6}$ natural size. The dotted lines indicate parts that are wanting.

Figure 3. Upper jaw of the same; natural size.

Figure 4. Lower jaw of the same, lacking a small piece at *a*.

Plate IV.—Figures 5 and 5a. Jaws of *Loligo pallida*; *a*, the rostrum or beak; *a b*, the cutting edge, with a notch at *b*; *b c*, the anterior edge of the alæ or wings; *d*, the frontal lamina in the upper jaw, or chin-portion (*mentum*) in the lower jaw; *e*, the palatine lamina in the upper jaw, or gular lamina in the lower jaw; twice the natural size.

Figure 6. Part of the lingual ribbon of *A. monachus*; enlarged.

Figure 7. Ditto of *Loligo pallida*; much more enlarged.

Figure 8. Ditto of *Loligo Hartingii*, copied from Harting; enlarged.

Figure 9. Caudal fin of *A. monachus* (No. 5); $\frac{1}{10}$ natural size.

Figure 10. Marginal ring of a sucker from one of the sessile arms; enlarged two diameters.

Figure 11. *a*, A large sucker; and *b*, a small marginal sucker from the tentacular arms of same; natural size.

Figure 12. Large sucker from tentacular arm of No. 4; natural size.

Figure 13. Part of the marginal ring of the same; enlarged.

[To be continued.]

ART. XVII.—*On the Mechanical Work done by a Muscle before Exhaustion;* by F. E. NIPHER, Assistant Professor of Physics in Washington University.

THE work done by a muscle may be classed under two heads: 1st. The exerting of any force (F) through any distance (D). To distinguish this kind of work from the other, we may call it dynamical work. It is measured by the product FD. 2d. Suppose an experimenter to hold a weight on his outstretched arm, and suppose furthermore that the experimenter is one of the weights of a large Atwood's machine, and is moved vertically with an accelerated motion. The dynamical work done by the arm before exhaustion is easily obtained from the equations of dynamics. If now the acceleration be-

comes zero, and the weight be simply held on the arm at a constant level, the arm very soon tires, showing that work is still done. This kind of work, which Prof. Haughton has called "statical" work, is purely molecular, and we hope to be able to show that it may be referred to and easily measured by the ordinary unit of mechanical work—the kilogram-meter.

In the supposed case,* if the acceleration becomes negative, the time of fatigue increases, until for an acceleration of minus (g), the time required to wholly fatigue the arm is infinite.

These considerations show, not only the reality of "statical" work, but also that it is an important element in all kinds of muscular work.

As the unit of statical work, we shall adopt the work done by the horizontally outstretched arm in sustaining a weight of one kilogram for one second upon a lever-arm of one meter. This work is evidently equivalent to the lifting of this same weight through a certain height.

As the unit of dynamical work, we shall take the dynamical work done by the horizontally outstretched arm, in lifting a weight of one kilogram, through a height of one meter, with an acceleration of one meter per second, on a lever-arm of one meter. The solution of the problem evidently consists in finding the relation between these two units.

In 1871 I published two series of experiments on this subject,† which consisted in lifting a weight w through a height h, in a time t, the intervals t being separated by equal intervals, during which the weight was allowed to drop, being caught on a cushion attached to the leg. In one series w was made variable, in the other series t varied. These experiments were repeated by Mr. Haughton, and in spite of my warnings in regard to the inaccuracy of the method, he has given them a "theoretical" reduction in his Principles of Animal Mechanics; London, 1873, pp. 462 and 475.

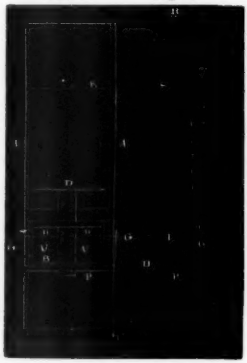
In order to eliminate the fatigue caused by the downward-plunging weight, the apparatus shown in figs. 1 and 2 was devised. B is a shelf armed with a plate of car-spring caoutchouc, for the support of the weight. This shelf is fastened by the iron bands I and a vertical back-piece V, to the slide U, being further supported by a cross-piece P. By using suitable blocks, G, the slide, may be raised to any desired height, which height is read off on a scale marked on the upright pieces A. D is used as a support for the arm during the interval of rest, and it can be adjusted to any desired height. S (fig. 2) is a horizontal string attached to the wire K, and to the wall of the

* We hope soon to be able to commence the series of experiments here indicated, the apparatus for which has been already devised.

† Hinrichs' "School Laboratory." 1871. p. 108.

room. R is an iron rod for holding the apparatus in position. The weight is a bucket of shot, provided with a stiff bail and wooden handle, so that during the lifting of the weight the line passing through the center of the hand and the center of gravity of the weight is a vertical. The experimenter stands to the right of the apparatus (as in fig. 2) and lifts the weight from the shelf B until his knuckles touch the cord S. (Back of hand turned upward.) The beginning and close of this interval of work is marked by the sharp click of a metronome, the time of whose beat is t. At the instant when the weight has reached the highest point, it is grasped by an assistant and lowered to the shelf, the arm of the experimenter being wholly relaxed, and resting upon the stiff bail of the bucket and the support D. The arm of the experimenter moves in the vertical plane which makes an angle of 45°, with the vertical plane passing through the centers of the shoulder joints.

Fig. 1. Fig. 2.



This process is kept up until the arm can no longer lift the weight to the required height. The determination of the number of lifts, n, should *never* be made by the experimenter, who should, furthermore, try to lose all estimate of time during the process. Only in this way can the mind be kept unbiased, so that the determinations are independent of each other.

In the earlier experiments it became evident that the arm not only grew gradually stronger, but also that it varied exceedingly in strength from day to day. In order to get some measure of the strength of the arm, I exhausted the arm each day of experiment with a 5·0 kgrs. weight, and reduced all of the experiments to the mean strength, as shown by the constant experiment. In this series, a weight, w, was lifted through a height, $h=0{\cdot}70$ meters, in a time, t, of 1·25 sec. The interval of rest was also 1·25 sec. The experiments were so arranged as to make the constant experiment come, alternately, at 11 A. M. and 4 P. M., the other experiments alternating in like manner. The weight, w, was changed each day of experiment, so that in the table below the numbers on the horizontal lines are arranged as observed, the order of time being indicated by the indices in the first vertical column.

The columns headed c are the number of lifts n' for the constant experiment $w=5{\cdot}00$. The mean value of c for the weights 3·0, 3·5, etc., 7·5, in all 100 experiments, is 35·79. The determinations of n for $w=7{\cdot}5$ and 8·0 were consciously bad,

as the arm was unable to manage such weights at such velocity, so that I was obliged to stop *before the arm was exhausted.**

RIGHT ARM. w variable. $h = 0{\cdot}70$ meters. $t = 1{\cdot}25$ sec.

w	2·00	c	2·50	c	3·00	c	3·50	c	4·00	c	4·50	c	5·00	c	5·50	c	6·00	c	6·50	c	7·00	c	7·50	c	8·00	c
☞	239	34	140	33	124	31	69	27	53	34	36	25	32	27	26	32	19	31	16	30	15	36	11	37	7	28
☜	479	35	186	30	144	32	62	32	57	28	32	25	26	28	25	28	19	30	16	24	11	25	7	26	6	26
☞	445	35	239	33	138	42	79	37	61	34	38	28	33	32	26	33	19	31	17	30	17	36	10	33	7	28
☜	707	39	271	33	158	38	79	34	66	35	45	30	38	37	27	30	24	38	19	39	16	40	10	32	8	26
☞	---	--	208	33	128	37	97	35	80	35	47	39	44	45	25	33	25	37	20	40	14	33	12	37	9	37
☜	---	--	221	31	122	33	89	31	55	33	44	37	28	28	28	36	23	32	20	36	14	34	12	31	9	37
☞	---	--	245	30	177	31	124	38	103	48	61	48	41	40	34	39	26	43	19	39	15	35	10	35	7	46
☜	---	--	520	38	180	39	117	42	71	36	71	42	46	37	34	43	26	42	18	43	15	41	11	37	6	37
☞	---	--	340	34	216	42	129	39	87	44	67	37	31	34	25	38	23	38	18	47	16	43	8	45	5	39
☜	---	--	---	--	178	44	100	34	66	46	52	33	43	44	32	44	23	36	16	36	13	41	9	37	7	33

The values of n' for $w < 3{\cdot}00$, were also rejected in the calculation of the constants, as with such light weights the work varies greatly with a slight variation of strength. [This will be fully discussed hereafter.] Taking the constant c as the measure of the strength, and assuming that the work done with any weight at different times is proportional to the strength, (which we will show to be true for the weights not rejected), and we have

Calling n'= observed number of lifts before exhaustion, and n=the same, reduced to the basis the mean strength 35·79

$$n = \frac{35{\cdot}79}{c} n'$$

from which formula we have the following values of n. The mean values of n are given in the eleventh line, and the prob-

	2·00	2·50	3·00	3·50	4·00	4·50	5·00	5·50	6·00	6·50	7·00	7·50	8·00
	251	152	143	91	56	51·5	42·4	29·1	21·9	19·1	14·9	10·7	8·9
	489	222	161	69	73	45·8	33·2	32·0	22·7	23·9	15·8	9·6	8·2
	454	259	118	76	64	48·5	36·9	28·2	21·9	20·3	16·9	10·8	8·9
	648	294	149	80	67	53·7	36·8	32·2	22·6	17·4	14·3	11·2	11·0
		225	124	99	82	43·1	35·0	27·1	24·2	17·9	15·2	11·6	8·7
		255	132	103	60	42·6	35·8	27·8	25·7	19·9	14·7	13·9	8·7
		292	204	117	77	45·5	36·7	31·2	21·6	17·4	15·3	10·0	5·4
		488	165	100	71	60·5	44·5	28·3	22·2	15·0	13·1	10 6	5·8
		358	184	118	71	64·8	32·6	23·6	21·7	13·7	13·3	6·4	4·6
			145	105	51	56·4	35·0	26·0	22·9	15·9	11·3	8·7	7·6
Mean	----	----	152·5	95·8	67·2	51·2	36·9	28·6	22·7	18·1	14·5	10·4	7·8
e	----	----	5·7	3·5	2·0	1·7	0·9	0·6	0·3	0·2	0·1	0·1	0·4

* This point is important. Try to lift 20 kgrs. in a second through 0·70 meters. You will fail to lift it once, and *yet not be exhausted.* The question of maximum velocity attainable with different weights is wholly different from the one under consideration. I think Mr. Haughton has overlooked this influence on his own experiments.

able error (e) of this mean is given in the twelfth line, and is calculated from the formula $e=\frac{2}{3}\sqrt{\frac{[d.d]}{v(v-1)}}$, where v = the number of observations and d= difference from the mean.

Assuming the arm to be a uniform cylinder, and denoting by α one-half the weight of the arm, and we have for any weight w the total dynamical work done before exhaustion.

$$\text{Total work} = (w+\alpha)h \,.\, n. \quad . \quad . \quad (1)$$

The value of α can be determined by direct weighing, as follows: Exhaust the arm thoroughly, then holding it in the same position as when lifting, extend the arm horizontally, resting the hand in the scale-pan of a spring-balance, the dial of which is turned from the experimenter. The reading off of the weight is done by an assistant. After a few minutes the muscles tire so that a practical experimenter can then gradually relax them fully. Untrained muscles, when thus tried, act involuntarily, and precise results can not be obtained. The value of α was thus determined twenty times, the values being here given.

α (obs.)				
1·46	1·42	1·63	1·42	1·46
1·42	1·58	1·42	1·48	1·40
1·42	1·52	1·48	1·58	1·50
1·50	1·49	1·62	1·58	1·57
				α=1·50

The mean is 1·50 kgr. with a probable error of 0·01 kgr. Hence (calling the total work=W), (1) becomes

$$W=(w+1{\cdot}50)\ 0{\cdot}70n.$$

Coördinating the values of W and w and the relation appears plainly hyperbolic. Hence the two most probable cases are

$$(w+\alpha)hn=\frac{c'}{(w+\alpha)^{v'}} \quad \text{---------- (2) and}$$

$$(w+\alpha)hn=\frac{c}{w^v} \quad \text{----------(3)}$$

where c and v are constants.

From these we readily have

$$\log(w+\alpha)+\log n=k'-v'\log(w+\alpha) \text{ --------(4)}$$
$$\log(w+\alpha)+\log n=k-v\log w \text{ ----------(5)}$$

These equations are of the form $y=k+vx$.

where y and x can be determined from the observations. They are given in the table below.

These values of x and y for eq. (4) and (5) are coördinated on the chart and a straight line, drawn as nearly as possible through the points, shows the functions to be linear in each case. v is

the change in y, for each unit of change in x, and is for eq. (4), 2·58. For eq. (5) it is 1·99, which is essentially 2·0.

w	$(w+a)$	n	$\log w$	$\log(w+a)$	$\log n$	$(\log w+a)+\log n$
3·0	4·5	152·5	0·4771	0·6532	2·1833	2·8365
3·5	5·0	95·8	0·5441	0·6990	1·9814	2·6804
4·0	5·5	67·2	0·6021	0·7404	1·8274	2·5678
4·5	6·0	51·2	0·6532	0·7782	1·7093	2·4875
5·0	6·5	36·9	0·6990	0·8129	1·5670	2·3799
5·5	7·0	28·6	0·7404	0·8451	1·4564	2·3015
6·0	7·5	22·7	0·7782	0·8751	1·3560	2·2331
6·5	8·0	18·1	0·8129	0·9031	1·2577	2·1608
7·0	8·5	14·5	0·8451	0·9294	1·1614	2·0908

As will be hereafter shown, the observations are most nearly represented by eq. (3). It is, however, impossible to decide between equations (2) and (3) with absolute certainty, until the experiments are repeated with other values of h and t. For the present we assume the equation

$$(w+\alpha)hn=\frac{c}{w^2} \qquad \text{(6)}$$

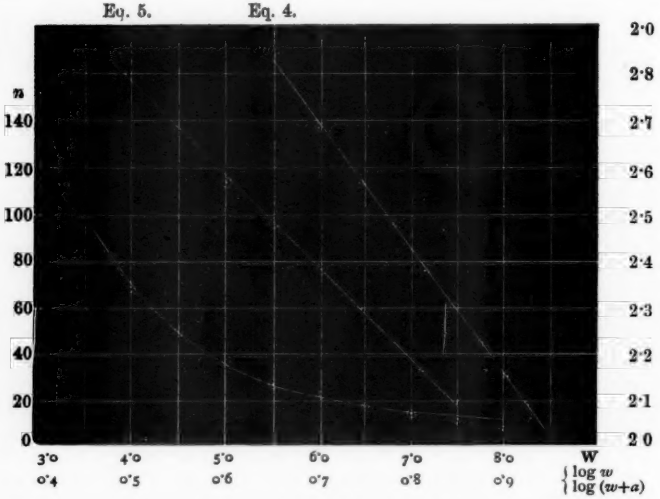
from which we readily have $w^3hn=c-\alpha w^2hn$,
which is of the form of $y=c+\alpha u$.

By the method of least squares, the values of the constants are more accurately determined, and are found to be

$$\alpha=1{\cdot}52 \qquad c=42{\cdot}61.$$

Solving (6) for n, and substituting the proper values, and we have the following comparison of the observed and calculated values of n. δn is n (calc.) —n (obs.). e is the probable error of n (obs.), also in per cent.

w	n (obs.)	n (calc.)	δn (%)	e (%)
2·50	283	242	—14·4	7·5
3·00	152·5	150·3	− 1·4	3·7
3·50	95·8	99·4	+ 3·6	3·6
4·00	67·2	69·2	+ 2·9	2·9
4·50	51·2	50·1	− 2·1	3·3
5·00	36·9	37·4	+ 1·3	2·4
5·50	28·6	28·7	+ 0·3	2·9
6·00	22·7	22·5	− 0·9	1·3
6·50	18·1	18·0	— 0·5	1·1
7·00	14·5	14·6	+ 0·7	0·7
7·50	10·4	11·9	+14·4	0·9
8·00	7·7	9·9	+28·5	5·2



The comparison of the calculated and observed values of n is very satisfactory. This is shown graphically on the chart, the small circles representing the observations. The values of δn, with the proper sign, are also there represented by the broken dotted line, the zero line being the horizontal line, which it repeatedly crosses. The observed value of α is 1·50. The calculated value is 1·52. The difference, 0·02 kgr., being 1·3 per cent of α observed.

Instead of eq. (6), Prof. Haughton has found the relation for the case here discussed, to be represented by the equation.*

$$(w+\alpha)^2 n = A \quad \ldots\ldots\ldots\ldots (7)$$

where for my right arm he finds the values of the constants to be $A=1000$ and $\alpha=1·0$. The experiments from which this formula was obtained are, as before said, unreliable, and the calculated value of α is 33 per cent less than the observed value. This relation would also demand that v', in equation (2), should equal unity. It was found to be 2·58.

In order to test the matter, experimentally, I performed two experiments as follows:

1. The arm alone ($w=0$) was lifted through 90° (from vertical to horizontal) in the time 1·25 seconds, the experiments being conducted exactly as described in my later experiments.

According to (7) for complete exhaustion $n=1000$
" (6) " " $n=\infty$

* Principles of Animal Mechanics. London, 1873. p. 463.

The arm was lifted 2000 *times without feeling any appreciable exhaustion.*

2. A weight $w=0{\cdot}50$ kgr. was lifted, as before, except that the weight was allowed to *drop* during the interval of rest, as in my earlier experiments.

It was lifted 1500 *times with very little of fatigue.*

According to (7) for complete exhaustion $n=$ 440
" (6) " " $n=12000$

In the latter case the time of exhaustion would be $8\frac{1}{3}$ hours. The work done before exhaustion would be 17,000 kgr.-meters. The daily labor of a working man is about 100,000 kgr.-meters. Considering these facts, and also the slight fatigue experienced by me, in experiment 2, I am convinced that my arm could work 8 hours and 20 minutes at the above rate, care being taken to avoid fatigue from other sources (standing on the feet in one position, etc.). It would, however, be a dangerous experiment.

Experiment 2 was also performed with a time $t=1{\cdot}164$ (the time taken in my earlier experiments, reduced by Prof. Haughton) with the same result.

This experiment also shows that eq. (2) does not represent this kind of work, for we deduce, from the chart, for the values of the constants

$$(w+\alpha)h \,.\, n = \frac{21240}{(w+\alpha)^{2{\cdot}58}},$$

making $w=0{\cdot}5$, and we have (since $\alpha=1{\cdot}5$ and $h=0{\cdot}7$) $n=$ 2500. The arm would be exhausted for 2500 lifts, which is hardly possible.

Anyone who will take the trouble to coördinate the corresponding values of c and n' as given in the first table, will see clearly that we were justified in retaining those values of n, and only those, which we there reduced. Each curve, however, gives evidence of the fact, that for any weight, n varies as some power of the strength.

We have made several series of experiments to investigate this point, determining the strength (s) of the muscle, by means of a dynamometer. Calling τ, the time of exhaustion I have found that

$$\tau=\alpha(s-\beta)^v,$$

where v is a function of the weight, α and β being also constant for the same weight. If the dynamometer really gave the absolute strength of the muscle, β would be equal to w. Precise values of these constants have not yet been obtained, and we therefore decline to discuss this equation any farther at present.

St. Louis, Oct. 31, 1874.

SCIENTIFIC INTELLIGENCE.

I. Chemistry and Physics.

1. *Constitution of Sulphurous Acid and Sulphites.*—Michaelis and Wagner have made some experiments to determine whether sulphurous acid possesses the constitution assigned to it by Strecker, $H.SO_2.OH$, or has the more usual formula $HO.SO.OH$. Since, if the former formula be correct, the two hydrogen atoms have different positions in the molecule, it is obvious that the replacement of one of them by another atom or atomic group must give rise to two isomers; while only one compound is possible when they are both replaced by the same radical. By this reasoning, only a single ethyl-ether of sulphurous acid can exist, its formula being $C_2H_5.SO_2.OC_2H_5$. But in fact two bodies having the composition of ethyl sulphite are known; one boiling at 161° and the other at 207°. The former is the true ethyl-ether of sulphurous acid. The latter, prepared by the action of sodium ethylate upon ethyl-sulphon-chloride, must from its mode of preparation be $C_2H_5.SO_2.OC_2H_5$. Since the two bodies are not identical, it follows that the only possible formula for the former is $C_2H_5O.SO.OC_2H_5$, corresponding to an acid of the formula $HO.SO.OH$. This view the authors confirmed by acting with phosphoric chloride upon the sulphurous ether produced when sulphur chloride acts on alcohol. Ethoxyl-thionyl chloride, $Cl.SO.OC_2H_5$, was obtained; and this, as shown by Carius, for thionyl chloride, treated with alcohol, yields ethyl sulphite. This result proves that the equivalence of sulphur is different in sulphuric and sulphurous acids, being in the latter a tetrad.—*Ber. Berl. Chem. Ges.*, vii, 1073, Sept., 1874. G. F. B.

2. *On the Organic acids of crude Petroleum.*—Hell and Medinger have examined an acid liquid, obtained by agitating the second running (specific gravity 0·875) in the distillation of heavy Wallachian petroleum with caustic soda, and treating the flocculent precipitate thus formed, after solution in water, with sulphuric acid. This oily acid liquid, which collects on the surface, called mineral-oil acid by the workmen, is a mixture of probably homologous acids, whose separation was exceedingly difficult, fractional precipitation yielding semi-fluid products, and distillation producing decomposition. Finally, an ethyl-ether boiling constantly at 236°–240°, was obtained as a colorless oily liquid, highly refractive, and having an agreeable fruity odor and burning taste. Upon saponification, and subsequent decomposition by sulphuric acid, a colorless acid of specific gravity 0·982 at 0° was obtained, which boiled constantly at 258°–261° without decomposition. It is a weak acid, its ammonium salt being decomposed by water in excess. The sodium and potassium salts closely resemble soft-soaps and could not be obtained in the solid form. Analysis gave as the probable molecular formula of the acid $C_{11}H_{20}O_2$. Its reactions show that it does not belong to either

of the three series of fatty acids at present known, and the authors believe it to be a member of a new series characterized by a peculiar mode of union of the carbon atoms.—*Ber. Berl. Chem. Ges.*, vii, 1216, Sept., 1874. G. F. B.

3. *On the Direct Synthesis of Methyl aldehyde.*—BRODIE has succeeded in effecting a direct synthesis of methyl aldehyde by passing the induction spark through a mixture of carbonous oxide and hydrogen. In his earlier experiments, in which equal (or nearly equal) volumes of the two gases were used, the only other product obtained was marsh gas; and this equally whether carbonous oxide or carbon dioxide was used. By a modification of the conditions of the experiment with carbon dioxide, he has effected the expected synthesis, the 188·6 volumes of gas remaining after the removal of the nitrogen, oxides of carbon, and a trace of oxygen, being composed of 183·2 of hydrogen, 0·2 of marsh gas and 5·2 of methyl aldehyde. This latter amount being equivalent to 2·76 per cent. The reaction appears to be as follows:

$$CO_2+(H_2)_2=COH_2+H_2O$$

Brodie believes that methyl aldehyde was formed in his former experiments, but was subsequently decomposed thus:

$$(COH_2)_2=CO_2+CH_4$$

Ann. Ch. Pharm., clxxiv, 284, Nov., 1874. G. F. B.

4. *Synthesis of an Isomer of Cane-sugar.*—The relation of the glucoses, or simple sugars, to the saccharoses, or compound sugars, is very obvious from their formulas; two molecules of the former by the loss of a molecule of water being united into one molecule of the latter. While it is quite possible by means of ferments, acids, and the like, to reverse this operation and produce the simple sugars from the compound ones, it has not hitherto been possible to build up the latter from the former. GAUTIER has now succeeded in withdrawing a molecule of water from two molecules of dextrose, one of the simple sugars, and thus forming a body having the composition of a compound sugar. The result was effected by dissolving perfectly pure dextrose in nearly absolute alcohol, cooling the solution by means of ice, and then passing very slowly through it a current of dry hydrochloric acid gas. After saturation, the liquid is evaporated in a vacuum, neutralized by barium carbonate and hydrate, treated with strong alcohol, again evaporated, treated with ether to remove a bitter substance, and finally dried in vacuo at 100°. A colorless body is thus obtained, analogous to gum and dextrin in appearance and taste, very soluble in water, and very hygroscopic. Upon analysis, its composition was found to be $C_{12}H_{22}O_{11}$. It has no sweet taste, is not precipitated by ammoniacal lead acetate, reduces the copper test only with difficulty and is dextrogyrate. It is not fermentable, and is decomposed again on heating its aqueous solution to 160°, into two molecules of a simple sugar; but this sugar appears not to be identical with the primitive dextrose, but to be analogous though not identical with inosite, reducing the

copper test but not being fermentable. The author suggests that the acetylic derivative of dextrose obtained by Schützenberger, $C_{12}H_{14}(C_2H_3O)_8O_{11}$, is the octacetylic derivative of this new compound.—*Bull. Soc. Ch.*, II, xxii, 145, Sept., 1874.

5. *On Maltose.*—SCHULZE has re-investigated and confirmed the statement of O'Sullivan that the action of malt-extract upon starch produces, not dextrose as had been supposed, but a compound sugar having the formula $C_{12}H_{22}O_{11}$, which he called maltose. This sugar reduces the copper test only two-thirds as actively as dextrose, but possesses a much greater rotatory power. Schulze's experiments were conducted with diastase, precipitated from malt-extract by alcohol. Starch paste treated with this, at a temperature of 60°, was rapidly saccharified. After concentration and precipitation of the dextrin by alcohol, the solution was evaporated to a syrup treated with alcohol, the clear solution poured off and evaporated over sulphuric acid. Crystals first appeared on the walls of the vessel, and finally the whole solidified to a crystalline mass. Recrystallized from water or alcohol, maltose retains crystal water, which is readily given up at 100°. In appearance, maltose resembles dextrose but is distinguished from it by its composition, its reducing power, and its action on a polarized ray. By boiling with dilute acids it is converted into dextrose. It is therefore an intermediate product between dextrin and dextrose.—*Ber. Berl. Chem. Ges.*, vii, 1047, Sept., 1874.

G. F. B.

6. *Synthesis of Leucic acid by means of its Nitrile.*—ERLENMEYER and SIGEL, by acting on amyl aldehyde—obtained from the alcohol produced by fermentation—with hydrocyanic acid, have produced the true nitrile of leucic acid, as a colorless, peculiarly smelling oil, insoluble in water. It cannot be distilled, being decomposed into the bodies from which it was formed. Potassium and sodium hydrates also decompose it similarly. Analysis gave it the formula $C_6H_{11}NO$. Fuming hydrochloric acid decomposes it, producing ammonium chloride and leucic acid. On the addition of water the latter product separates as an oil, sinking to the bottom. It is removed by means of ether, which leaves it on evaporation as a syrup, from which after a time large transparent crystal plates of the acid separate, having the composition $C_6H_{12}O_3$. There can be no doubt therefore that leucic acid is thus constituted:—

$$\left.\begin{matrix}CH_3\\CH_3\end{matrix}\right\}CH\text{---}CHOH\text{---}COOH.$$

Ber. Berl. Chem. Ges., vii, 1109, Sept., 1874. G. F. B.

7. *Relation of Phthalic acid to Anthraquinone.*—During the preparation of sulpho-anthraquinonic acid in a Basle alizarin manufactory a considerable quantity of a crystallized sublimate was obtained in large colorless needles, which was soluble in water and crystallized therefrom in brilliant plates. WEITH and BINDSCHEDLER examined it and proved it to be phthalic acid, by conversion into the anhydride and into calcium and barium phthalates,

as well as into phthalimide. Further investigation showed that anthraquinone carefully purified from phthalic acid, heated with four parts of fuming sulphuric acid for six hours to 270°, gave colorless needles of phthalic anhydride, nearly an inch in length. Hence anthraquinone must be regarded as the ketone of phthalic acid, the decomposition taking place as follows:

$$C_6H_4\begin{cases}CO\\CO\end{cases}C_6H_4+2SO_2\begin{cases}OH\\OH\end{cases}=C_6H_4\begin{cases}COOH\\COOH\end{cases}+C_6H_4\begin{cases}SO_2OH\\SO_2OH\end{cases}$$

Ber. Berl. Chem. Ges., vii, 1106, Sept., 1874. G. F. B.

8. *Simple preparation of Uræmatin from Hæmatin.*—Hoppe-Seyler has identified the coloring matter which he had produced by acting upon hæmatin, the coloring matter of the blood, with reducing agents, especially tin and hydrochloric acid, with that obtained from the urine by Jaffe and called urobilin, and that produced from bilirubin by Maly by the action of sodium amalgam. Hence it follows that the coloring matter of normal fæces, and of urine, is only a reduction derivative of the coloring matter of the blood, and that the biliary coloring matters bilirubin and biliverdin are intermediate stages in this reduction. The principal normal coloring matter of the urine, however, is not urobilin but uræmatin; but Hoppe-Seyler states that this is easily derived from urobilin, and that too, by reduction. This discovery opens the way for the solution of interesting questions on the disintegration of the blood-corpuscles under pathological conditions.—*Ber. Berl. Chem. Ges.*, vii, 1065, Sept., 1874. G. F. B.

9. *Asparaginic acid as a product of Pancreatic digestion.*—Since asparaginic acid has been proved to be a product of the splitting up of the albuminates, both vegetable and animal, it occurred to Radziejewski and Salkowski to ascertain whether natural ferments such as that of the pancreatic juice, produced asparaginic acid in their action upon blood-fibrin. Fresh blood-fibrin was digested at 40°–50° with the pancreas of the ox for several hours. Upon testing the solution obtained, asparaginic acid was detected; its identity being established by conversion into the copper salt and by elementary analysis. The formula $C_4H_7NO_4$ requires 36·09 of carbon and 5·26 hydrogen. Analysis gave 35·76 carbon and 5·68 hydrogen.—*Ber. Berl. Chem. Ges.*, vii, 1050, Sept., 1874. G. F. B.

10. *Sympathetic Vibrations*—M. E. Gripon has studied at length the influence exercised on the vibrations of a column of air by neighboring sonorous bodies. He concludes that the pitch of a vibrating mass of air is raised by bringing near its orifice an elastic membrane or a second mass of air which alone would give the same note. A similar effect is produced, but in a less marked manner if the second mass of air or membrane, has a higher pitch than the first. The pitch of a membrane, on the other hand, is lowered by bringing a solid body near it. In order that a layer of air, bounded on one side by a membrane and on the other by a plane parallel to it formed by a solid free around its edge, may be reinforced by a given sound, its thickness must be proportional to its length;

further, this thickness depends on the ratio which exists between the sound proper to the membrane and the given sound, and also on the nature and dimensions of the membrane.

We can, with sounding tubes with flute embouchure, reproduce the principal phenomena obtained with singing flames.

The pitch of a pipe falls when we bring a solid body near its orifice. This flattening is still produced when the pipe is the center of a solid plane which extends indefinitely around the pipe. The change in pitch is proportional to the number of vibrations of the pipe, and within certain limits nearly proportional to the breadth of the solid rim surrounding the pipe; it is, on the contrary, constant starting from a certain breadth, and then proportional to the diameter of the pipe.—*Ann. Chem. et Phys.*, iii, 343–390. E. C. P.

11. *Electrical Resistance.*—M. Benoit has measured with great precision the electrical resistance of various metals at temperatures from 0° to 860°. He employed both the method of the differential galvanometer and of the Wheatstone's bridge, and for each metal has measured several specimens. The mean of these is given in the following table, the second column giving the resistance of a wire one meter long and having a cross section of one mm. in Ohms, and column three the same quantity in Siemens units. Column four gives the resistance compared with silver:

Metal.	Ohms.	Siemens.	
Silver, A	·0154	·0161	100·
Copper, A	·0171	·0179	90·
Silver, A (1)	·0193	·0201	80·
Gold, A	·0217	·0227	71·
Aluminum, A	·0309	·0324	49·7
Magnesium, H	·0423	·0443	36·4
Zinc, A., at 350°	·0565	·0591	27·5
Zinc, H	·0594	·0621	25·9
Cadmium, H	·0685	·0716	22·5
Brass, A (2)	·0691	·0723	22·3
Steel, A	·1099	·1149	14·0
Tin	·1161	·1214	13·3
Aluminum Bronze, A (3)	·1189	·1243	13·0
Iron, A	·1216	·1272	12·7
Palladium, A	·1384	·1447	11·1
Platinum, A	·1575	·1647	9·77
Thallium	·1831	·1914	8·41
Lead	·1985	·2075	7·76
German Silver, A (4)	·2654	·2775	5·80
Mercury	·9564	1·0000	1·61

A, annealed; H, hardened; (1) silver ·75; (2) copper 64·2, zinc 33·1, lead 0·4, tin 0·4; (3) copper 90, aluminum 10; (4) copper 50, nickel 25, zinc 25.

These results, which are all taken at 0°, agree closely with those obtained by other observers. M. Benoit has extended his observations to a range of temperature much greater than those previ-

ously employed for this purpose. He wound the wire around a clay pipe enclosed in a muffle and immersed the whole in a bath of water, mercury, sulphur or cadmium, which was kept at the boiling point by a Perret furnace. Constant temperatures of 100°, 360°, 440° and 860° were thus obtained. Various temperatures below 360° were also obtained by a mercury bath. The measures were also corrected for expansion. Plates annexed to his memoir, presented to the Faculty of Sciences of Paris, show the results graphically. They show that the resistance increases regularly for all metals like tin, lead and zinc up to their point of fusion. This increase, however, differs for different metals. We notice that tin, thallium, cadmium, zinc, lead, are found together in the upper part of the plate; at 200° to 230° their resistance has doubled. Below them are iron and steel; for the last the resistance doubles at 180°, quadruples at 430°, and at 860° is about nine times that at 0°. Palladium and platinum, on the other hand, increase much less and only double their resistance at 400° to 450°. Gold, copper and silver form an intermediate group. In general the conductibility decreases more rapidly in a metal the lower its point of fusion. Iron and steel are an exception to this rule. In alloys the variation is always less than in their constituents, and this is especially the case with German silver.—*Bib. Univ.*, cciii, 284. E. C. P.

12. *Reflection by Glass.*—Dr. P. Glan has measured the amount of light reflected by a plate of glass and compared the results with those given by theory. The apparatus employed consisted of a doubly-refracting prism attached to the collimating lens of an optical circle, so that its principal section was parallel to the slit. The objective of the observing telescope carried a Nicol with a divided circle reading to minutes; and the spectral analysis of the light was effected by the prisms of a Hoffman spectroscope set on the table of the apparatus. The slit was divided into two parts by a strip of tintoil, whose breadth was such that the ordinary image of one-half and the extraordinary image of the other were in exact contact, consequently by turning the Nicol the two adjacent spectra could always be brought to precisely the same degree of brightness. Owing to the dispersion of the doubly-refracting prism, perfect contact was only possible for one color, but the spectra were of such a length as to render the contact close enough over a space of considerable width; by turning the doubly-refracting prism this contact could be effected in any part of the spectrum. A rectangular prism was placed over the lower half of the slit and reflected through it the light of a petroleum flame, while in front of the upper half a small spectrometer was placed, by which the light reflected from the glass was passed through the apparatus. By turning the Nicol the two images were rendered equal, and each observation was repeated five times, using the light of wave-length equal to that of thallium.

In the following table the first column gives the angle of incidence, the second the mean of twelve observations of the amount

reflected by a prism of crown glass, the third the amount reflected according to Fresnel's formula, and the fourth and fifth the observed and computed reflections for a similar prism of flint glass. The incident beam is in all cases taken as 100.

	Crown glass.		Flint glass.	
I.	Obs.	Calc.	Obs.	Calc.
30°	5·5	5·9	7·0	7·1
40	7·2	7·3	8·4	9·3
50	10·4	11·4	12·0	13·3
55	13·3	14·1	16·1	16·2
60	17·4	17·9	20·3	20·3
65	23·1	22·9	25·4	25·7
70	29·3	30·2	32·7	33·0

The indices of refraction were found from the angle of total polarization, since Seebeck has shown that the density and, therefore, the refraction of the surface may be considerably altered by grinding. For the crown glass this method gave $n=1{\cdot}507$, and for the flint glass $n=1{\cdot}577$. It is evident that the agreement with theory is all that could be expected, since the individual observations differ in some cases as much as four per cent from one another.—*Monats. Acad., Berlin*, 1874, p. 511; *Phil. Mag.*, xlviii, 475.

E. C. P.

13. *Electrical Polarization.*—M. G. Quincke describes in detail a great number of experiments on the electrical currents accompanying the non-simultaneous immersion of two mercury electrodes in various liquids, and has arrived at the following conclusions:

If two mercury electrodes, connected by the wire of a multiplier, be immersed one after the other in any liquid which is a conductor of electricity (water, alcohol, saline solutions, etc.), an electric current is observed passing from the freshly wetted mercury surface through the liquid to the other mercury surface. The strength of the current diminishes as the resistance of the liquid column between the electrodes is increased. The electromotive force varies with the nature of the liquid and increases as the concentration diminishes, in some cases amounting to 0·6 of a Volt. The electromotive force increases if the boundary surface of mercury with the surrounding liquid in the last immersed electrode is more quickly produced. It soon, however, reaches a maximum, especially in the case of viscous liquids like glycerine. The cause of these currents is probably the alteration in molecular condition (change of density or concentration), which is gradually accomplished in the liquid near the surface of contact after the wetting. Similar effects are obtained with solid metals, as with mercury. The currents formed with acids are due chiefly to chemical action, and are, therefore, secondary phenomena. The surface tension may be either increased or diminished and may change its sign with the direction and duration of the current. The disturbances in capillarity can not be accounted for by electrolysis.—*Pogg. Annal.*, cliii, 161–203; *Phil. Mag.*, xlviii, 479.

E. C. P.

II. Geology and Natural History.

1. *On the Cosmical dust which falls to the earth with atmospheric precipitation;* by A. E. Nordenskiöld.—In the early part of December, 1871, the region about Stockholm was visited by a snow fall of unprecedented magnitude. Nordenskiöld availed himself of this opportunity to investigate whether the apparently pure snow did not contain particles of foreign matter. A cubic meter of the snow was collected and melted with all possible precautions: it was taken, moreover, during the latter part of the storm (which lasted several days), when the atmosphere must have been purified from all accidental dust. Notwithstanding this, a residue was obtained consisting of a black powder, from which, when heated in a matrass, a fluid product was distilled off, and which gave, on being ignited, a reddish-brown ash. It contained also magnetic particles, which gave the reaction for iron.

The experiment was not in all respects satisfactory, as it was at least supposable, notwithstanding the precautions taken, that the organic matter might have had its origin from the thousands of chimneys about Stockholm, and that the metallic iron might have been derived from the roofings of the houses, covered up though they were at the time by several feet of snow. In order to obtain more positive results, the experiment was repeated in a remote forest district of Finland. A large body of snow was collected with care and melted. The result in this case also was a sooty powder, consisting of a black coal-like mass with minute magnetic particles, which proved to be metallic iron. The quantity of material obtained was too small to allow of its being tested for cobalt or nickel.

Still more conclusive evidence to the same point was obtained in the winter of 1872, when Nordenskiöld, in connection with the polar expedition, spent the winter on the northern coast of Spitzbergen. At this point (lat. 80° N.) most remote from all human habitations, the investigations were further carried on. The snow which covered the drift ice, which itself had come from much higher latitudes, was found to be thickly dusted with minute black particles; they rested in part on the surface, and were in part enclosed in the icy snow mass some inches below. The dust was black when collected, but became gray on drying. It proved to contain metallic particles which, with sulphate of copper, gave a precipitate of metallic copper. The experiment was repeated later, the material being obtained from a layer consisting of a granular crystalline mass of altered snow, covered by eight millimeters of old hardened snow, and that in turn by 50 mm. more, which was loose and had recently fallen. At this place it was estimated that about 0·1 to 1·milligram of the magnetic particles were contained in a square meter. Enough of the material was obtained to allow of some qualitative experiments, and it was proved to contain, beside metallic iron, also phosphoric acid, cobalt and probably nickel. The portion insoluble in acids consisted of a firm, angu-

lar, colorless powder, in which some fragments of diatoms were observed.

This material has much resemblance to the remarkable dust found by Nordenskiöld scattered on the surface of the ice in the interior of Greenland, as also at a distance of 30 miles from the coast, and to which he gave the name *cryoconite.* This consisted for the most part of minute angular crystalline grains, which were colorless and transparent, with fragments possibly of feldspar and augite crystals, and some black magnetic particles. In an analysis the cryoconite was proved to consist of silica, alumina, oxide of iron, manganese, magnesia, potash, soda, with traces of chlorine and organic matter, and to give the oxygen ratio for the protoxyds, alumina, silica and water, 2 : 3 : 14 : 1. Its specific gravity is 2·63, and the crystalline form is monoclinic.

Nordenskiöld shows that the cryoconite must have had either a cosmical origin, or have come from Jan Mayen, or else some unknown volcanic region in the interior of Greenland, while the presence of cobalt, and probably nickel, would seem to prove that a part of the dust at least had a cosmical origin. He finally comes to this conclusion—that small quantities of a cosmical dust, containing metallic iron, cobalt, nickel, and phosphoric acid and also a carbonaceous organic matter, falls upon the earth along with atmospheric precipitation.—*Pogg. Ann.*, cli, 154, 1874.

[The dust, or cryoconite, has nearly the composition of an oligoglase-trachyte.—E. S. D.]

2. *On Middle Park Mineral Coal;* by E. J. Mallett. (From the *Rocky Mountain News*, Denver, Colorado, of Nov. 19, and dated Territorial School of Mines, Nov. 17, 1874.)—I have examined specimens of coal from Middle Park, and herewith communicate the results, which may be of interest to your readers.

The coal presents somewhat the appearance of bituminous coal of the older epochs, and does not have much tendency to slack or break to pieces. When pulverized, it loses its jet-black color, assuming a dark vandyke-brown. * * * * * * *

Although it might be classed as caking bituminous coal, it differs in the following respects from coals of that class:

First. In the nature of the residue left after distillation—a true coke, something similar to that which would be derived from distilling sugar, but too porous and crumbling to support burden in a furnace.

Second. In the large amount of gas and tarry oil produced.

Third. In the brown color of its powder.

English cannel coal was placed in another retort and heated simultaneously with the Middle Park coal, in order to compare roughly the relative amounts of gas. Apparently the domestic coal gave about thirty per cent more gas, of high illuminating power, and very much more liquid distillate than the English coal. When the coal is boiled with concentrated solution of potash, it does not affect the color of the solution. Lignitic or brown coal colors potash solution brown.

Considering all of its properties, I may say that it possesses much in common with the recently discovered mineral called *albertite*, a species of solidified petroleum, and also with what is known as *torbanite*. These two varieties are highly valued by gas manufacturers, who mix from five to twenty per cent of these bituminous compounds with less bituminous coal, thereby greatly increasing the yield and quality of the gas. It resembles the former in the large amounts of gas and tarry oil it yields (which may prove as valuable as that derived from albertite), but differs from it in being heavier—the specific gravity of the albertite being 1·090, while this is 1·323,—also in yielding no soluble products when treated with bisulphide of carbon, spirits of turpentine, ether, etc. From torbanite it differs, in not crackling in the fire, in being much heavier, and in melting and intumescing when heated. Analysis shows it to contain in one hundred parts 6·02 per cent of water and moisture, 39·95 per cent of volatile matter (gas and tarry oil), 54·03 per cent of fixed residue, consisting of coke and ash.

As much confusion exists in the nomenclature of the mineral fuel of Colorado, I would propose, as I recently did to Professor Hayden, to give up entirely the term lignite as a special class name. If we call our mineral fuel lignite, we must conclude that lignite can be either bituminous-caking, bituminous-non caking, anthracite, or possess an organic structure. The term lignite should be dropped, as being inapplicable when applied to our mineral fuel as a class.

The coal from Middle Park is of the class caking-bituminous, and being a peculiar variety, might be distinguished as "Byerite."

3. *Volcanic Phenomena of County Antrim and adjoining Districts, Northeastern Ireland;* by Prof. EDWARD HULL. (Proc. Brit. Assoc., Aug., 1874.)—The Antrim igneous rocks, which include those of the Giant's Causeway, are referred to the closing part of the Eocene period and the whole of the Miocene. The earlier of the ejected rocks—probably later Eocene—are stated to be trachytic lavas; they have a feldspathic base and contain crystals of sanidin and also grains of quartz. They come from many vents and have a thickness in some places of 600 feet. Over these there are sheets of augitic lavas (which are separated often by layers of reddish clayey or soil-like material), and also beds of volcanic ashes. The ash-beds of Ballypalidy, the Causeway and other places contain fossil plants of Miocene character. Following the ash depositions, after a considerable interval, there were other augitic eruptions in vast sheets, covering in some places beds of lignite and others of pisolitic limonite. The whole thickness of the basaltic sheets of Antrim is estimated to be 1100 feet; or 1300 feet for the basaltic and the underlying trachytic sheets. This, though so large, is far short of the thickness on Mull, which is made by Geikie to be 3,000 to 4,000 feet. In both regions the thickness has been reduced by denudation. Mr. Hall states that the thousands of dikes in northeastern Ireland—whose aggregate breadth would be several thousand feet—were made without any crumpling of the strata.

The augitic rock consists of crystalline grains of augite and labradorite with grains of titano-ferrite [a titaniferous magnetite?], and often also of chrysolite. Owing to the amount of iron in the rock, its decomposition has produced beds of pisolitic limonite. No distinct remains of volcanic cones can now be distinguished in the region.

4. *Report of the Geological Survey of Missouri, including field-work of* 1873–4, with 91 Illustrations and an Atlas; by GARLAND C. BROADHEAD, State Geologist. 734 pp. 8vo. Jefferson City, 1874.—We cite the following from this valuable Report.

The "Historical Notes," with which the Report opens, show that lead-mining was begun in Missouri in 1719, but was prosecuted only on a small scale for many years. In 1823 the annual yield was reported at 3,000,000 pounds; in 1873 it reached 27,676,320 pounds. The first iron was made in 1816; the first zinc, in 1867.

The Lower Drift includes large boulders of granite, red quartzyte, greenstone, etc., which increase in quantity and size as we go north. In Sullivan County, a granite boulder measures 20 by 24 feet. The Missouri River seems to limit their southern extension; for only small and scattered ones are seen farther south, and these mostly rounded, as if by stream action. It is very probable that, anterior to the deposit of this rounded drift, but subsequent to the age of deposit on the highest land, an immense lake covered the larger part of the State, including all of North Missouri, St. Louis County, the counties on the Missouri River above Moniteau County, and a portion of the border counties, while all Central and Southern Missouri formed an extensive area of dry land. We have evidence in North Missouri of great erosion previous to the existence of this immense lake.

The total area of the Missouri coal-field is estimated at 23,100 square miles.

The term Choteau Group is used as the equivalent of Kinderhook, on the ground of priority; but, since it was originally applied to but one of the three beds now included, while the "Kinderhook," from the first, was used to include all three—Choteau Limestone, Vermicular Sandstone and Lithographic Limestone—with their equivalents elsewhere, this use seems liable to cause needless confusion. The "greater thickness and better development" of the group in Missouri does not appear to us sufficient reason for replacing a name in itself unobjectionable.

After brief summaries of facts concerning caves, water-supply, soils, economic minerals and rocks, and topographical features of the southwest coal field, there follow over three hundred pages of county reports. The following chapters, on lead, zinc and iron ores, by Schmidt and Leonhard, give many interesting details concerning the occurrence and relations of these ores. At Joplin and Oronogo, the galenite is frequently associated with bitumen, which sometimes entirely impregnates the ore. This bituminous galenite has a darker appearance than the common ore, being often deep black in color. Bitumen is of quite common occurrence in the

Joplin mines. Analyses of galenite, from nine different localities, show an average of a little over an ounce of silver to the ton.

At the Oronogo mines, specimens have been obtained, which show very plainly the gradual change from galenite to cerussite. Crystals or masses of galenite, imbedded in altered and softened limestone, seem to have been coated at first by a compact layer of cerussite. Through this layer, solutions containing carbonic acid penetrated and gradually dissolved the galenite, precipitating a part of the lead as carbonate on the inside of the coating, but carrying most of it out of the shell, either to deposit it immediately or to carry it away. When this process has been continued for some time, we find a round or oval shell of compact and generally crystalline cerussite, of the size of a walnut, or sometimes much larger, containing a dark, transparent, watery fluid, in which a piece of galenite, smooth and rounded, but irregular in shape, is lying loose. If this process continues, the galenite dissolves entirely, and a cavity lined with cerussite crystals is the final result.

In Southwest Missouri, chert (of Subcarboniferous age) occurs in heavy beds. The greatest observed thickness is 105 feet. This is here one of the principal ore-bearing rocks, containing galenite in numerous sheets or seams. It is evident, in all places, that the galenite was formed after the chert had been formed and hardened and in many places broken up. The limestone has undergone, in many places, a process of dolomization. Solutions penetrating it have dissolved the carbonate of lime and partly replaced it with carbonate of magnesia. With this change a contraction is connected; cracks are opened in the mass, and filled with crystallized dolomite. The change was always begun either in fissures or on the surfaces of layers or of broken off blocks, and gradually proceeded toward the interior of the rock. It seems to have preceded the formation of the ore, and to have continued during the whole process. The deposition of zinc ores, especially calamine, and of galenite, is evidently intimately connected with the dolomization. At Granby, the alteration extends through horizontal zones of irregular outline, generally from two to six feet high, above which the solid beds are undisturbed. In the Joplin districts, the ore-deposits are mostly in the form of "runs," extending principally in one horizontal direction, rarely more than five feet wide, and limited in height by the layers of chert above and below the limestone layer, in the crevice of which the run originated. Another mode of alteration of these rocks consists in a gradual solution and removal of the limestone, either unaltered or more or less dolomized. This was undoubtedly effected by water containing carbonic acid. Large caves are produced by this dissolving process, whenever the layers of chert above do not happen to break down.

The lead ores of Central Missouri are mostly in the Lower Silurian limestones, though partly, also, in the Subcarboniferous, as are all those of the southwest portion of the State. None of the deposits as yet worked have proved to be paying to a greater depth than about 80 feet below the surface. This fact is independent of

the geological position of the deposits. It is a remarkable fact, also, that while barite is entirely missing in the southwestern region, this mineral is, in this central region, a nearly constant associate of the galenite in the Carboniferous as well as in the Silurian rocks. These facts suggest the idea, as Mr. Schmidt observes, that the occurrence of lead ore, with its associates, is not exclusively dependent on the geological formation, but that its deposition may have taken place simultaneously in similar rocks belonging to different geological periods, which rocks happened to be under similar conditions at the time of deposition. If this suggestion is correct, it would throw the origin of all the galenite of the central and southwestern lead regions into a later period than the Subcarboniferous. As galenite is also found in the seams and partings of the coal strata in Simpson's coal-mine, situated in the lead district of southern Moniteau County, which strata, although belonging to a separate basin, must be supposed to have been formed simultaneously, or nearly so, with the North Missouri coal-field, we may conclude that the galenite is of much later origin, even, than the Coal-measures.

The Atlas accompanying the Report contains geological maps of Cedar, Barton, Vernon, Bates, Howard, Madison, Jasper and Newton Counties, with uncolored maps of the lead districts, and three sheets of parallelized sections of the Coal-measures. These last seem to us objectionable, in that they give but little information, while involving great expense. The *field-notes* thus published might better be reserved for the special study of their author, and only the *thoroughly digested general section* sent to the printer.

5. *Geological Survey of Victoria.* Melbourne, 1874. (London: Trübner & Co.).—Two volumes of Reports on the Geology of Victoria have recently been issued: one, a Report of Progress, by R. Brough Smyth, Secretary for Mines for the Colony, with additional reports on the mineral resources of Ballarat, by R. A. F. Murray, and on certain Coal-fields; and the other, Decade I, on the Paleontology of Victoria, by Frederick McCoy. The plants of the Coal-fields are mostly similar to those of Australia; but no species of *Glossopteris*, the very common Australian genus, have yet been found, and three are mentioned of the genus *Zamites*. *Glossopteris Browniana*, the most abundant Australian species, has, however, been reported from Southeastern Tasmania, along with *Pecopteris Australis*, a plant of both the Australian and Victoria Coal-fields. Professor McCoy makes all these Coal-measures Mesozoic. While no Lepidodendrids occur in these Coal-measures, a species is reported from a sandstone of Gippsland, which is very similar to the European Carboniferous *Lepidodendron tetragonum* Sternb. These beds are supposed to be Subcarboniferous.

The most interesting discovery mentioned in the Reports is that of the occurrence of many American species of Graptolites in the slates of Victoria. Mr. McCoy has identified 12 to 14 species of this group; and, of these, Decade I contains descriptions and figures of the following, which are described by Hall from the

Quebec group of the Lower Silurian: *Phyllograptus typus* Hall, *Didymograptus mucronatus* Hall, *Diplograptus pristis* (*Prionotus pristis*, var. B., Hall), *D. bicornis* Hall, *Graptolites fruticosus* Hall, *G. quadribrachiatus* Hall, *G. bryonoides* Hall, *G. octobrachiatus* Hall, *G. Logani* Hall. The last four are very common species both in Canada and Victoria, though rare in Europe.

6. *Report upon Vertebrate Fossils discovered in New Mexico, with descriptions of new species;* by Prof. E. D. COPE, Paleontologist. 18 pp. 8vo. Geogr. Expl. and Surveys west of the 100th Meridian, First Lieut. G. M. WHEELER, Corps of Engineers, U. S. Army, in charge.—Professor Cope states that the Eocene discoveries show that an Eocene lake extended over the part of New Mexico now drained by the tributaries of the Chama River on the east and the San Juan on the west. The Mammalian remains differ from those of the Fort Bridger Eocene in the absence of species of the genus *Palæosyops*, and its replacement by *Bathmodon* Cope, and by the presence of only one small *Hyrachyus*. Four new species of Toxodonts are among the discoveries reported; they are referred to the new genera *Ectoganus* and *Calamodon*.* The teeth of six or seven species of sharks and one *Ostrea* have been found with the Mammalian remains.

7. *Twenty-sixth Annual Report on the New York State Museum of Natural History;* by the Regents of the University of the State of New York. Transmitted to the Legislature, May 2, 1873. 192 pp. 8vo. Albany, 1874.—The series of Reports on the New York State Museum, which has now reached its 26th number, is of great value to science on account of the descriptions of new species contained in it, and especially of species of fossils by Professor JAMES HALL. The volume just issued contains descriptions of over 50 species of Bryozoans and Corals of the Lower Helderberg group, by Professor Hall; besides, also, Entomological Contributions, No. III, by J. A. LINTNER, including notes on the larves or transformations of various insects, and descriptions of some new species; and also a report of the Botanist, C. H. PECK, containing among its tables a list of plants hitherto unreported from the State, with various notes.

8. *Wind-drift erosion: Note by* G. K. GILBERT. (Communicated.)—At the Hartford meeting of the American Association for the Advancement of Science, a paper was presented by the writer upon "Erosion by Sand in the Western Territories," and an abstract of the same is already in type for the forthcoming volume of the Proceedings. One of the topics discussed was the degradation of desert plains by drifting sand—a degradation evinced by the peculiar worn surfaces of the pebbles which strew the plains. The hard pebbles exhibit a high polish; the softer parts of the heterogeneous are deeply scored; and pebbles of limestone are carved with an arabesque plexus of furrows. These phenomena were briefly noted by Dr. J. S. Newberry in 1861 (Geol.

* According to Professor Marsh, this genus is identical with his *Stylinodon*, described in this Journal in vol. vii, p. 532, May, 1874, where its resemblances to *Toxodon* were mentioned.—EDS.

Ives' Exped., pp. 17 and 24), and his observation was cited as the first on record. I have recently had my attention called to the fact that the phenomena were earlier observed by Prof. W. P. Blake, who published descriptions in this Journal and elsewhere in 1855, prior to the organization of Lieut. Ives's Expedition. (Pacific Railroad Rept., v, 108, 230 and 232; Am. Jour. Sci., 2d ser., xx, 180; Proc. Amer. Assoc. Adv. Sci., 1855, p. 218.) His description, which is more extended than Dr. Newberry's, touches the majority of the features to which I called the attention of the Association at Hartford; and to him belongs the credit of having first discovered and truly explained the facts.

9. *Zircon-syenyte of the Canaries.*—M. Meunier mentions in the Comptes Rendus (lxxix, p. 594, Sept., 1874) the occurrence of zircon-syenyte in the Canary Islands, the rock being identical with that long known from Scandinavia.

10. *Text-book of Geology, designed for Schools and Academies;* by JAMES D. DANA. 2d edition, 358 pp. 8vo. Illustrated by 400 woodcuts. New York and Chicago, 1874. (Ivison, Blakeman, Taylor & Co.)—This Geological Text-book of Professor Dana has been thoroughly revised, and thereby adapted to the arrangement of the new edition of the Manual and to the present state of geological science. The work is somewhat enlarged by the addition of new facts and illustrations, but without adding to the number of pages, the page having been increased in size to balance the additions, and also—as is true equally of the much-enlarged Manual—without an increase of price. The book is well printed on good paper.

11. *New American Geological Reports.*—The following Reports have been received, and will be noticed in another number of this Journal.

The Geology of New Hampshire, C. H. HITCHCOCK, State Geologist, and J. H. HUNTINGTON, Principal Assistant. Part I, Physical Geography. 668 pp. royal 8vo, with many plates and wood-cuts.

Contributions to the Fossil Flora of the Western Territories: Part I, *The Cretaceous Flora;* by LEO LESQUEREUX. 136 pp. 4to, with thirty lithographic plates, constituting volume VI of the quarto Reports of the United States Geological Survey of the Territories, F. V. Hayden, U. S. Geologist in charge. Department of the Interior.

First Annual Report of the Geological and Agricultural Survey of Texas; by S. B. BUCKLEY, A.M., Ph.D., State Geologist. 142 pp. 8vo. Houston, Texas, 1874.

Geological Survey of California, J. D. WHITNEY, State Geologist. *Contributions to Barometric Hypsometry*, with tables for use in California. 88 pp. roy. 8vo. 1874. Also map of California and Nevada. 1874.

The Surface Geology of Ohio: from vol. ii, of the Reports of the Geological Survey of Ohio; by J. S. NEWBERRY.

12. *Memoirs of the K. K. geologische Reichsanstalt.*—The following very fully illustrated memoirs in 4to, have been recently published by the K. K. geologische Reichsanstalt, Vienna.

Die Fauna der Schichten mit *Aspidoceras acanthicum ;* by Dr. M. Neumayr. 1873.

Ueber einen neuen fossilen Saurier aus Lesina ; by Dr. A. Kornhuber. 1873. The species is named the *Hydrosaurus Lesinensis.*

Ueber die triadischen Pelecypoden-Gattungen *Daonella* & *Halobia ;* by Dr. E. M. v. Mojsvár. 1874.

Ueber die palæozoischen Gebilde Podoliens und deren Versteinerungen ; by Dr. A. v. Alth. 1874.

Die Cephalopodenfauna der Gosauschichten in den nordöstlichen Alpen ; by A. Redtenbacher. 1873.

13. *Manual of Determinative Mineralogy, with an Introduction on Blow-pipe Analysis ;* by GEORGE J. BRUSH, Professor of Mineralogy in the Sheffield Scientific School. 8vo. New York, (John Wiley & Son.)—Professor Brush calls his work a compilation based on the tenth edition of Von Kobell's Tables. But, while this is true of it, he has contributed largely to the work from his own long labors in blowpipe mineralogy. Moreover, the species, with their blowpipe characters, and also, in additional columns, the several physical characters, have been arranged in a series of extended tables, which will be found exceedingly convenient by the student and sure of leading him, if he faithfully uses it, to the name of any mineral in hand. All species are included in it, even those recently announced.

This volume, as the Preface states, constitutes the Determinative Part of Dana's System of Mineralogy.

14. *Pharmacographia : a History of the Principal Drugs of Vegetable origin met with in Great Britain and British India ;* by F. A. FLUCKIGER, Ph.D., Professor in the University of Strassburg, and DANIEL HANBURY, F.R.S., &c. London : Macmillan & Co., 1874. pp. 704, 8vo.—A standard work like this has long been wanted ; and to Mr. Hanbury, botanists and druggists have been looking for it. The part taken by his German colleague is not specified ; but the result is in every way satisfactory. It is not a treatise on Materia Medica, at least in the modern sense ; the therapeutical applications of the drugs are indicated only in a general way ; still less is it concerned with pharmaceutical details and manipulations. Nor is it a Medical Botany, although accurate botanical knowledge is turned to the best account ; and the arrangement follows the natural orders of the plants yielding the drugs. It is the drugs themselves that are identified and described, rather than the plants that produce them. Their botanical origin is briefly discussed ; their history is next given, generally with fulness and freshness, condensing the results of much research and literary labor ; chemical composition is noted, but with no superfluity of symbolic formulas ; under "Production and Commerce" much statistical and trade information is given ; medical and economical uses are indicated ; the nature of the principal adulterations noted, and the means of detecting them pointed out ; and, finally, occasional substitutes, not of the nature of adulteration, are specified. In describing a root, bark, or the like, the microsopic

structure plays an important part, and is duly attended to. It has well been said that this can be made clear and explicit only by means of figures; but figures in sufficient number to be of much account would greatly increase the size and enhance the price of the work. A hand-book like this must do without them. A. G.

15. *Hymenomycetes Europœi, sive Epicriseos Systematis Mycologici editio altera;* scripsit ELIAS FRIES (Sumptibus auctoris). Upsaliæ, 1874.—The preface to this classical volume (of 755 8vo pages) is dated August 15, 1874, upon the venerable author's eighty-first birthday. He began to publish upon these *Fungi* sixty years ago, has all along been the acknowledged head and master of this recondite department, as well as one of the most learned and critical of Phanerogamous botanists; and his wonderful powers and sure judgment appear to be almost unabated. The full index fills nearly 50 pages. A. G.

16. *Miocene Fossil Plants of Greece.*—In the *Revue Bibliographique* of the Bull. Bot. Soc. France, 1874, p. 114, an abstract is given of a critical examination of Fossil Plants of Koumi, in Eubœa, by Count Saporta, a paper contributed to the *Ann. Sci. de l'Ecole Normale Supérieure*, ser. 2, t. 2. The main interest lies in the discovery of a Miocene flora, rich in peculiar species, and allied on one hand to the present Mediterranean vegetation, on the other to that of South Africa. It has *Widdingtonia*, *Podocarpus*, *Cussonia*, *Myricæ*, and *Anacardiaceæ*, &c., of the Cape types, and an *Encephalartos*, the first fossil *Cycadea* clearly made out as of an existing genus. It contains, moreover, *Glyptostrobus Europœus* of Heer, which passes insensibly into the existing representative, *G. heterophyllus* of China; and a *Sequoia* (*Fournalii*), which so closely approaches *S. sempervirens* of California, "que l'on ne saurait marquer aucune divergence sensible entre ces deux espèces." A. G.

17. *Mace.*—The mace of nutmeg, once taken as the type of an *arillus*, was, a good while ago, distinguished as a *arillode*, or false aril, by Planchon, on finding that it developed from the micropyle, while a true aril is a growth from the hilum or summit of the funiculus. Hooker and Thomson's statement, that the mace develops from both the micropyle and the hilum, has been confirmed by Baillon (*Comptes Rendus*, 78, p. 779, abstracted in *Rev. Bibliogr.* of Bull. Bot. Soc. France, l. c.). The consequence is that the distinctions between *arillus* and *arillode*, *caruncle* and *strophiole* become not exactly superfluous, but systematically unimportant. A. G.

18. *Organogeny of the Andrœcium and its bearings upon natural affinities*, by M. CHATIN.—A long and important series of papers in the Comptes Rendus, t. 78, 1874, of which a full and clear abstract is given in the Revue Bibliographique, above cited, occupying six pages. A. G.

19. JOHN TRAHERNE MOGGRIDGE'S name was misprinted *Maggridge* in the January number, p. 69. He was, as we learn, a grandson of *Dillwyn* of Swansea; and so his taste for natural history came by inheritance. A. G.

20. *On the effects of certain Poisons on Mollusks;* by WILLIAM NORTH RICE, of Middletown, Conn. (From the Proceedings of the American Association for the Advancement of Science, Portland Meeting, August, 1873.)—Professor Rice closes his paper on this subject with the following paragraph:

Among the most interesting results of the experiments was the observation that certain poisons which act with extreme violence upon the mammalia, are very feeble in their action on the mollusca. This is especially true of hydrocyanic acid and woorara. Specimens of *Ilyanassa obsoleta*, immersed in dilute hydrocyanic acid on Friday, showed somewhat feeble signs of life on the following Tuesday. A specimen of *Lunatia heros*, into which a quantity of woorara had been injected, was found the next day to show no sign of any injury. Indeed, both of these poisons seemed to produce death very little sooner than the animals would have died in stale water. The sudden introduction of a large amount of carbonic acid in the manner which has been described, seemed to produce no decided effect. On the other hand, chloral hydrate seems to be very suddenly fatal, the animals treated with it becoming instantly contracted, and not resuming their activity when kept for a number of hours in sea water. Cyanide of potassium is similar in its effects, though not quite so instantaneously fatal. The effects of quinine are similar, though less energetic. Chloroform produces instantaneous contraction, and probably death; but, as the animals treated with this poison were not afterward kept for a time in pure sea water to give them an opportunity to revive, it is not certain that they were really dead.

21. *On the mode in which Amœba swallows its Food.*—Professor LEIDY remarked that he had supposed that Amœba swallows food by this becoming adherent to the body, and then enveloped, much as insects become caught and involved in syrup or other viscid substances. He had repeatedly observed a large Amœba, which he supposes to be *A. princeps*, creep into the interstices of a mass of mud and appear on the other side without a particle adherent. On one occasion he had accidentally noticed an Amœba, with an active flagellate infusorium, a Urocentrum, included between two of its finger-like pseudopods. It so happened that the ends of these were in contact with a confervous filament, and the glasses above and below, between which the Amœba was examined, effectually prevented the Urocentrum from escaping. The condition of imprisonment of the latter was so peculiar that he was led to watch it. The ends of the two pseudopods of the Amœba gradually approached, came into contact, and then actually became fused—a thing which he had never before observed with the pseudopods of an Amœba. The Urocentrum continued to move actively back and forth, endeavoring to escape. At the next moment a delicate film of the ectosarc proceeded from the body of the Amœba, above and below, and gradually extended outwardly so as to convert the circle of the pseudopods into a complete sac, inclosing the Urocentrum. Another of these crea-

tures was noticed within the Amœba, which appeared to have been inclosed in the same manner.

This observation would make it appear that the food of the Amœba ordinarily does not simply adhere to the body, and then sink into its substance, but rather, after becoming adherent or covered by the pseudopods or body, is then inclosed by the active extension of a film of ectosarc around it.—*Proc. Acad. Nat. Sci. Philad.*, p. 143.

22. *On the Motive Power of Diatoms.*—Professor LEIDY made some remarks on the moving power of Diatoms, Desmids, and other Algæ. While the cause of motion remains unknown, some of the uses are obvious. The power is considerable, and enables these minute organisms, when mingled with mud, readily to extricate themselves and rise to the surface, where they may receive the influence of light and air. In examining the surface-mud of a shallow rain-water pool, in a recent excavation in brick clay, he found little else but an abundance of minute diatoms. He was not sufficiently familiar with the diatoms to name the species, but it resembled *Navicula radiosa.* The little diatoms were very active, gliding hither and thither, and knocking the quartz sand grains about. Noticing the latter, he made some comparative measurements, and found that the Naviculæ would move grains of sand as much as twenty-five times their own superficial area, and probably fifty times their own bulk and weight, or perhaps more.—*Ibid*, p. 113.

23. *The Common Frog;* by ST. GEORGE MIVART. Nature series. Macmillan & Co., 1874.—This little treatise contains not only a large amount of information concerning the comparative anatomy of frogs, but also numerous illustrations of the anatomy of other animals, and of man, introduced for the sake of comparison with the corresponding parts of the frog. It is illustrated by eighty-eight cuts, most of them excellent. V.

24. *Animal Mechanism: a Treatise on Terrestrial and Aerial Locomotion;* by E. J. MAREY. With one hundred and seventeen figures. International Scientific series. D. Appleton & Co., New York, 1874.—In this work the Subject of animal locomotion is treated in a very clear, analytical and concise manner, while the numerous illustrations render even the most complicated mechanisms easily understood. The first part of the book treats of the various forces and organs concerned in locomotion; the second treats of terrestrial locomotion; the third, of the flight of insects and birds. The author presents in this work the results of his own extensive investigations of this subject, and gives full accounts of the numerous mechanical contrivances by which the wings, legs, and bodies were made to record their own motions. Many of the tracings are reproduced in the cuts. Several machines, so constructed as to imitate faithfully the essential motions and effects of the wings of insects, are described and figured. The entire subject is so well explained and illustrated that the book cannot fail to be interesting and instructive, even to non-scientific readers. V.

III. Astronomy.

1. *The Transit of Venus, Dec.* 8, 1874.—The following notices present a summary of the stations at which successful observations of the Transit were made so far as information has yet been received. The stations are arranged in the order of latitude:

In the Northern Hemisphere.

1. *Tschita.* Lat. 52° 0′ N., long. 7h. 34m. E. Russian station. Contacts observed and four series of measures with heliometer.
2. *Nertchinsk.* Lat. 51° 18′ N., long. 7h. 58m. E. Russian station. Three contacts observed, and two diameters and twenty distances of the planet measured with heliometer.
3. *Kiachta.* Lat. 50° 20′, long. 7h. 6m. E. Russian station. Eight photographs taken.
4. *Habarovka.* Lat. 48° 16′, long. 8h. 58m. E. Russian station. First two contacts and some chords observed.
5. *Jassy.* Lat. 47° 3′, long. 1h. 50m. E. German station. Last external contact observed.
6. *Wladiwostok.* Lat. 43° 7′, long. 8h. 47m. E. American and Russian station. First and second contacts observed, 13 photographs taken, and numerous distances of the two limbs were measured.
7. *Port Possiet.* Lat. 42° 42′, long. 8h. 43m. E. Russian station. Two interior contacts observed and 38 photographs taken.
8. *Orianda.* (North of the Crimea?) Russian station. Satisfactory observation of last two contacts.
9. *Pekin.* Lat. 39° 54′, long. 7h. 46m. E. American and French station. First and second contacts observed and photographs taken.
10. *Tschifu.* Lat. 37° 30′, long. 8h. 5m. E. German station. The observation of contact, the heliometer measurement, and the photographs succeeded splendidly.
11. *Teheran.* Lat. 35° 37′, long. 3h. 25m. E. Russian station. Observations successful.
12. *Yokohama.* Lat. 35° 36′, long. 9h. 19m. E. Russian station. Observation successful.
13. *Kobe.* Lat. 34° 40′, long. 9h. 1m. E. French station. Successful observations.
14. *Nagasaki.* Lat. 32° 45′, long. 8h. 39m. E. American and French station. Second contact observed well, first and third contacts observed through clouds. 60 good photographs. 150 micrometric measurement of cusps, separation of limbs and diameter of Venus.
15. *Ispahan.* Lat. 32° 40′, long. 3h. 27m. E. German station. 19 photographs taken.
16. *Cairo.* Lat. 30° 6′, long. 2h. 5m. E. English station. Last two contacts well observed.
17. *Suez.* Lat. 29° 58′, long. 2h. 10m. E. English station. Last two contacts observed satisfactorily.
18. *Roorkee.* Lat. —, long. —. East India Co. station. 100 photographs taken.
19. *Thebes.* Lat. 25° 43′, long. 2h. 10m. E. English station. Last two contacts well observed. 50 photographs taken. Russian station. Splendid weather; very important observations.
20. *Honolulu.* Lat. 21° 18′, long. 10h. 31m. W. English station. First two contacts well observed. Photographs unsatisfactory. The complete disc of Venus was seen twelve minutes before the internal contact. The first contact was observed at 3h. 7m. 1s.; the time computed in the Eng. Naut. Alm. was 3h. 5m. 18s; O−C = +1m. 43s. Second contact obs. 3h. 35m. 55·7s.; comp. 3h. 33. 0s; O−C = +2m. 55·7s.

In the Southern Hemisphere.

1. *Sydney.* Lat. 33° 31′ S., long. 10h. 5m. E. English Observatory. Observations satisfactory.
2. *Adelaide.* Lat. 34° 40′, long. 9h. 15m. E. English Observatory. Last two contacts well observed. The third contact was observed at 3h. 4m. 43·4s.; the time computed by the American Commission was 3h. 5m. 49s.; O−C = −1m. 6s. Fourth contact obs. 3h. 34m. 7·5s.; comp. 3h. 35m. 39s.; O−C=−1m. 32s.
3. *Melbourne.* Lat. 37° 49′, long. 9h. 40m. E. English Observatory. Observations successful.
4. *Queenstown*, New Zealand. Lat. —, long. —. American station. Observations successful. Ingress observed and 237 photographs taken.
5. *Hobart Town.* Lat. 43° 0′, long. 9h. 49m. E. American station. 113 photographs taken.
6. *Christ Church*, New Zealand. Lat. 43° 20′, long. 11h. 31m. E. English station. Failure from clouds.

2. *Aurora Australis at Melbourne, Victoria.*—Traces of the aurora were seen on the evenings of the 7th and 8th of March; there were a few faint pink streamers on the 7th at 7.30 P. M. It was also seen on the 8th at Cape Otway and Port Albert, and on the evening of the 10th at Mount Macedon.—*Monthly Record during March, 1874, under the Superintendence of R. L. J. Ellery, Government Astronomer, Melbourne.*

IV. Miscellaneous Scientific Intelligence.

1. *Royal Society.*—The Copley medal of the Royal Society has been awarded to Prof. Louis Pasteur, "for his researches on Fermentation and Pebrine;" and the Rumford medal to J. Norman Lockyer, "for his spectroscopic researches on the Sun and on the Chemical elements."

2. *Proceedings of the Centennial of Chemistry.*—Numbers 2, 3 (for August and September) of volume v of the American Chemist are occupied with the proceedings of the Centennial meeting at Northumberland. They contain the address of Prof. H. H. Croft, on the life and labors of Priestley; of Dr. H. Coppée at the grave; of T. Sterry Hunt, on the century's progress in chemical theory; of J. Lawrence Smith, on the century's progress in industrial chemistry, and of B. Silliman, on American contributions to chemistry. To the last the author has added, with much labor, a list of papers published by American authors, bearing on chemistry, physics, and the chemical composition of minerals, and including also references to many papers on other topics.

Report of the Commissioners of Agriculture for 1873. 496 pp. 8vo. Washington, 1874.

Half-hour Recreations in Popular Science. No. 11. The Transmission of Sound by the Atmosphere, by John Tyndall. Gigantic Cuttle-fish, by W. S. Kent. 32 pp. 8vo. Boston. (Estes & Laureat.)

Half-hours with Insects. Part 5. Insects of the Pond and Stream, by A. S. Packard, Jr. pp. 129–160, 12mo. Boston. (Estes & Laureat.)

Tidal Researches, by William Ferrel, A.M., Assistant U. S. Coast Survey. Appendix to the U. S. Coast Survey Report for 1874. A profound work, the result of many years labor.

Topographical Survey of the Adirondack Wilderness of New York for the year 1873; by Verplanck Colvin. Transmitted to the New York Legislature, April 21, 1874. 306 pp. 8vo, with many valuable maps. Albany, 1874.

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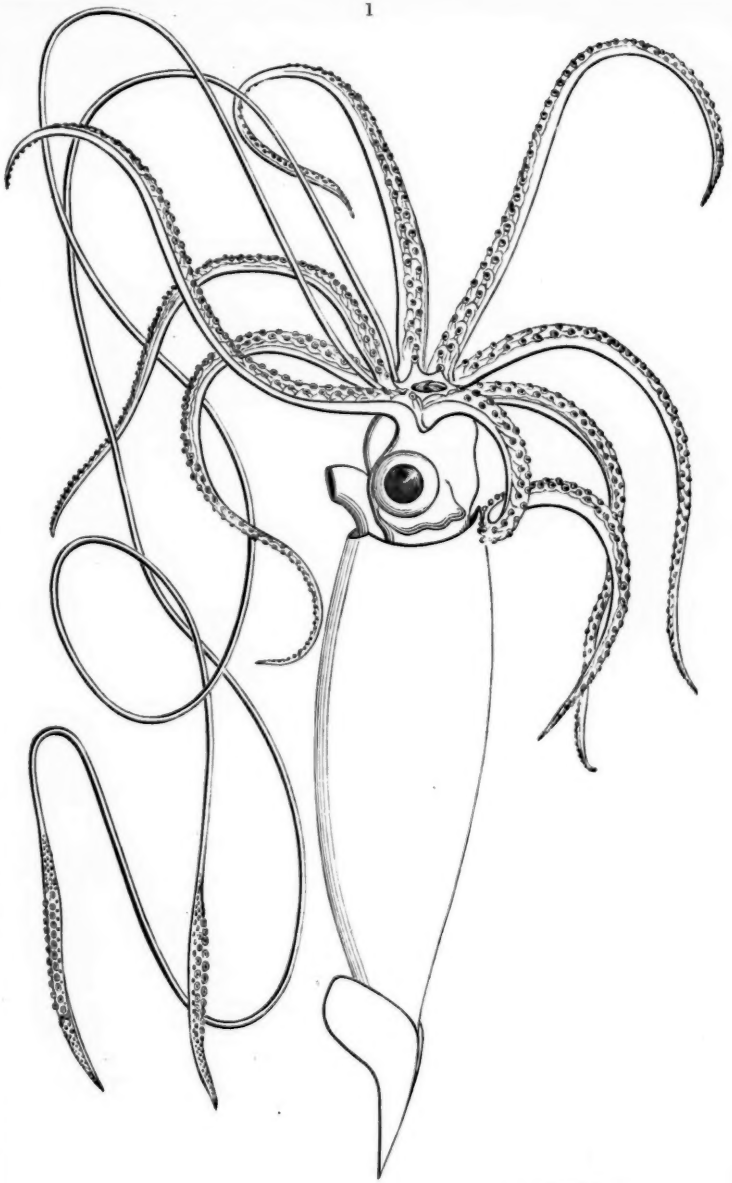

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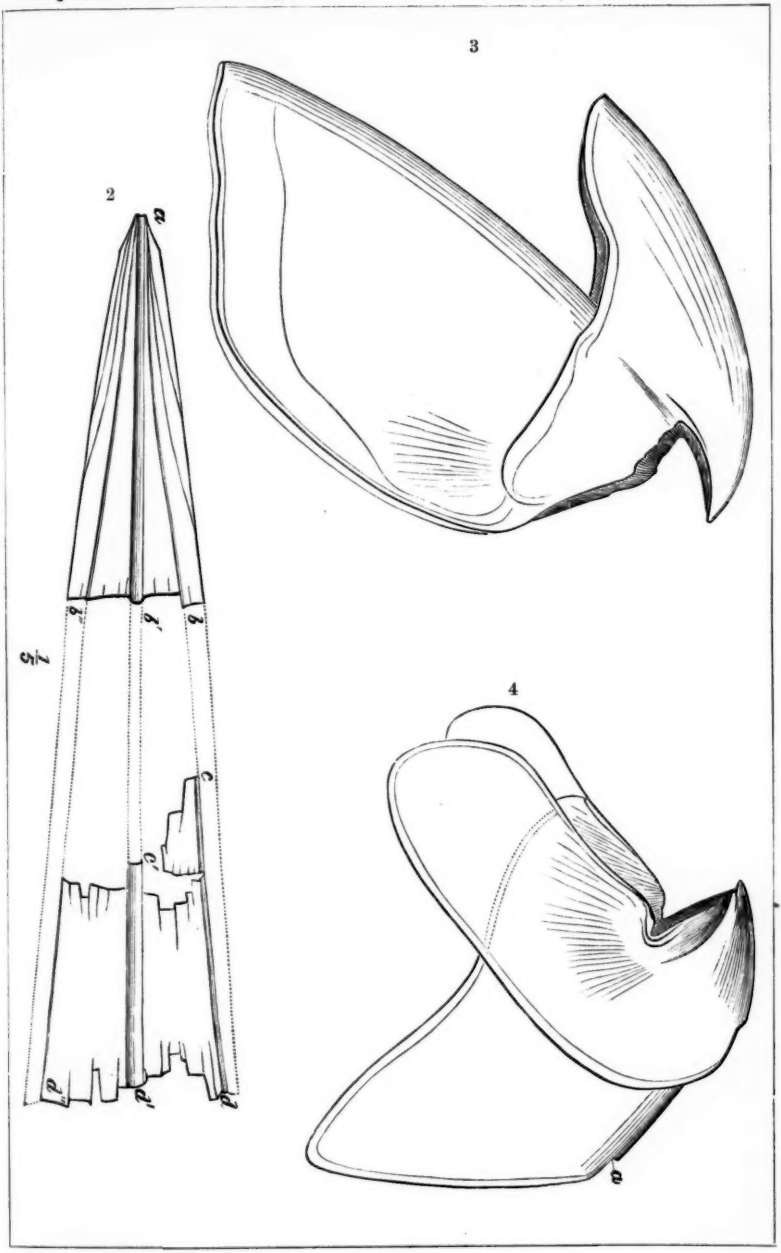
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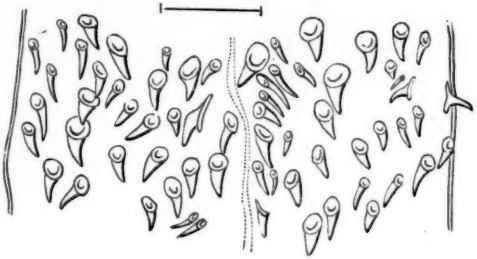

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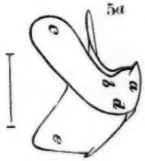

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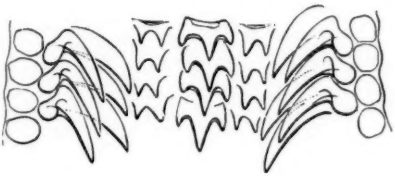

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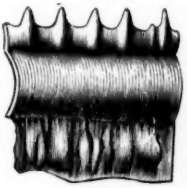

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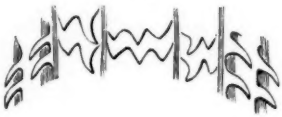

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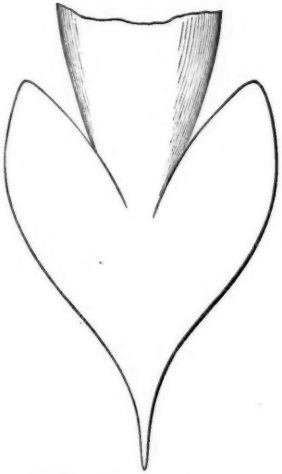

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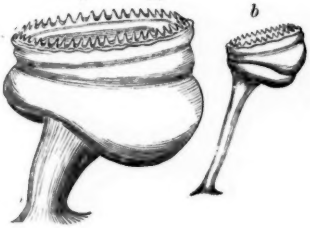
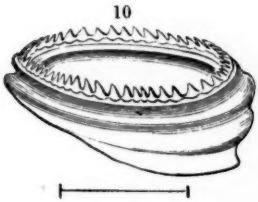
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